

A
COMPLETE TREATISE
ON THE
NATURE, SYMPTOMS, AND CURE,
OF

Lues Venerea,

HISTORICAL, THEORETICAL, PRACTICAL,
AND ORIGINAL.

A NEW EDITION, AMENDED AND CORRECTED

By JESSÉ FOOT, Esq. SURGEON.

*Quandoquidem sapiunt aliene ex ore, petuntque
Res ex auditu, potius quam sensibus ipsis.*

LUCRETIUS.

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P R E F A C E.

I HAVE made it a matter of my choice thus cheerfully and consciously to offer this Treatise on LUES VENEREA, and have thought it most prudent* that I should in this Preface give a short exposition on the manner in which I have arranged the contents. ASTRUC was the only Author who collected evidence on the Origin of the Disease in Europe; without reference to him, doubts might have been entertained as to that question. ASTRUC collected his authorities from all the literature of every country, both medical and historical, and my gratitude to this indefatigable and ingenuous Author would not permit me to be silent, in support of his fame, when I see it attacked as it has been, and is, by the wayward opinions, not proofs, of mistaken publishers. ASTRUC has also another claim on posterity for having been the first Author who arranged the Symptoms of Lues Venerea so fully and methodically, it

being the result of his observation. Whosoever is experienced in the practice of treating Lues Venerea will find that the Disease is, in all its gradations, such a disease as he has so learnedly and naturally described. To ASTRUC alone, therefore, these two high qualities are due.

Many prefatory remarks would be superfluous, as I was obliged to go into explanation at some length upon a theory new to most medical students, and most certainly originating from my own perception. In order that the attention should not be withdrawn, I have set apart the Nature and Action of Lues Venerea, and considered it separately from the method of Cure, that both might be studied distinctly. In the practical part, I have aimed to be as explanatory as I possibly could, avoiding always unnecessary prolixity. I have made no otherwise use of medicines, but in general, as adjuncts to assist or to substitute for mercury: I have employed the whole of my practical judgment in laying down rules when to persist in, when to desist from, and when, in case of necessity, to take mercury up again. I thought it would encumber a practical volume to print *anateria medica*, when the "London Pharmacopœia" is always at hand. *Ne quid nimis* is an old and good adage.

Diseases in the Urethra, Prostate, Bladder, and Kidnies, which belong not to this work, are already treated on at full length by me in two separate publications. But, above all, I beg it to be understood, that this performance is theoretically and practically my own, that it is the result of my own observation, and of my own practice. Theory is knowing what to do, practice is doing what is known. When theory and practice go not hand in hand, “ ’tis easier to teach twenty what is fit to be done, than *to be one of that twenty* to follow one’s own teaching.” I most cheerfully submit this volume, not with that apology of toil and trouble which some Authors pretend, who, like porters, on delivering a very light weight, take out their pocket-handkerchiefs to wipe the sweat from off their foreheads, in order to play upon generosity. I am not indebted to the work of others, nor do I enjoy the good of mercury, then cavil the conditions. I have not come down at stated times to rummage the Bookseller’s compter for materials to work upon. When I think of such Authors, it raises a smile, though mixed with pity,—it reminds me of an animal called by naturalists the soldier. This little creature, says the Historian, is passionately fond of a shell; but, not

being supplied with one by nature, has recourse to the deserted shell of some other. I have seen these harmless reptiles, continues he, come down once a year from the mountains, rank and file, cover the whole shore, and ply busily about, each in quest of a shell to please it. Nothing can be more amusing than their industry upon this occasion. One shell is too big, another too little; they enter, and keep possession sometimes for a good while, until one is, at last, found entirely to please. When all are thus properly equipped, they march up again to the mountains, and live in their new acquisition till under a new necessity of changing.

Dean-street, Soho,

Nov 1819.

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A
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CHAPTER I.

ON THE ORIGIN OF LUES VENEREA.

ALTHOUGH an investigation of the origin of the LUES VENEREA in Europe may not be, at this time, considered as positively necessary for defining the symptoms of the Disease, or for assisting the cure, yet, upon more general reflection, such an investigation must be judged to be an interesting point of speculative philosophy, as it involves, by the enquiry, the nature of diseases produced through climates and habits of life, which are indigenous in one or other part of the globe, either at periodical seasons, or continually.

The substance of what has been advanced, to invalidate the late origin of Lues Venerea, obliges the Author to produce upon this business something

more than a few ambiguous scraps from obscure, and the most part unpolished writings, and which most evidently relate to other distempers.

The writers seem to have shaken together in a large bag what scattered wisdom they could lay hold of on the antiquity of Lues Venerea prior to the return of C. Cæsar, but, as ill fortune would have it, the scraps that came up nearest were all out of place.

ASTRUC, the learned and judicious Historian, may call him, on the subject of Lues Venerea, has enquired into the history of diseases in the days of the Greeks and Romans, but has not been able to produce any Author which satisfactorily corroborates the opinion of those who are desirous to attribute its origin to another and prior cause, — to make it an ancient disease with another name, and to establish Lues Venerea an original, venomous, morbid, disease of Europe.

It is scarcely three hundred and thirty years since this distemper first shewed itself in Europe, and we have extant upwards of four hundred different Treatises upon this subject, by Authors of great reputation, besides the notice which has been taken of it in almost every book that has been written on Physick.

Then it must be admitted at least to be extraordinary, that they should all, to a man, for full two thousand years, omit to mention Lues Venerea, so violent, so remarkable and common a disorder, if this distemper had been among them, and which,

when undisguised by mercury, would not have been misunderstood. How comes it to pass, if the disease did exist before the æra of the return of Columbus's crew, that no one Author should have written specifically upon it before that æra, and that so many have since?

I shall now proceed to the authorities for the strong ground entertained, that Lues Venerea first made its appearance in Europe towards the latter end of the year 1494. This may be learned from the joint testimonies of all the physical writers who flourished then in Italy, and have treated of the origin of the distemper.

I particularly request the attention of the Reader to the most important of all points in this question, namely, that the Authors and Historians I quote lived at the time of the return of Columbus, between the year 1494 and 1566 at the remotest, but chiefly between 1494 and 1536. Whereas these collectors of scraps are not able to produce before the year 1494 one single volume written specifically
Lues Venerea.



CHAPTER II.

MEDICAL TESTIMONIES ON THE ORIGIN OF
LUES VENEREA IN EUROPE.

JOSEPH GRUNPECH, a German Physician, published, in the year 1496, "*Tractatum de Pestilentiali Scorraë sive Mala de Frantzoz,*" in which he affirms, that it was a disease so lately inflicted on mankind, that it seemed to be a plague sent down from Heaven, — that it was a new kind of disease, hateful to Nature, — a most horrid and terrible prodigy, unheard of, hated, and altogether unknown to mortals before that time.

ALEXANDER BENEDICT, of Verona, who was Physician in the Venetian army which Charles the Eighth of France destroyed in the Battle of Fornova, in the year 1495, and therefore had the opportunity of observing the first appearance of this new disease, asserts in his work, "*de omnibus morbis,*" published in 1496, that "by the Venereal Contact, a new French disease, or at least one that was unknown to former Physicians, owing to the pestiferous aspect of the stars, had burst in upon them from the West;" — and in another part of his work, that "the French Disease, a new plague which was sprung up in the world, contracted by

lying together and contact, was reckoned, in his time, incurable."

NICOLAS LEONICENUS, of Vicenza, Professor of Physic at Ferrara, the famous reviver of Greek Physic, in a Treatise which he wrote in the year 1496, *de Morbo Gallico*, observes first, that "new diseases had appeared in Italy, which were unknown to former ages, after the manner of the Lichenis, which, according to Pliny, *Hist. Natural. lib. 16*, were never known before the time of Claudius." Then he proceeds thus :

"Something like this has happened in this age, for now a new disease, of an unusual nature, has attacked Italy, and several other countries; however, this disease has obtained no proper name hitherto by our present Physicians, but they commonly call it the French Disease; as if the contagion had been imported by the French into Italy, and that this country was infested both by the Disease and the arms of France at the same time. I, for my part, am forced to believe (nor indeed can I conceive the case to be otherwise) that this infectious disease, which has lately sprung up, has harassed this present age, as it never did any former one."

CORADINUS GILINUS, in his "*Opusculum de Morbo Gallico*," begins thus :

"Last year (1496) a very violent disease attacked great numbers of people, both in Italy and the other side of the mountains, which the Italians call the French Disease, affirming, that the French intro-

duced it into Italy; which the French call the Italian or Neapolitan Disease, because they say they were first infected in Italy, and especially at Naples, with this cruel plague; or because the Disease appeared first in Italy at the time of the passage of the French over the mountains. And as this Disease is yet unknown to the Moderns, and there have been and still subsist great debates about it among Physicians, I have therefore determined to write something about it."

BARTHOLOMEW MONTAGNANA, the younger, of Padua, wrote, in the year 1449, *Physical Advice* to Peter Zenus, of Venice, for the most illustrious and most reverend Bishop and Viceroy of Hungary, who at that time was ill of this Disease. "This Disease (he affirms) was unknown to Hippocrates, Galen, Avicenna, and other ancient Physicians; and therefore its proper name was not known. For, provided Avicenna (proceeds he) had known it, he would certainly have given some account of it, and called it by its proper name, as he has done to a great many others. Whence he concludes, that the French Disease, which appears now-a-days, did not exist in the times of Galen, Hippocrates, and Avicenna."

GASPER TORELLA, of Valencia, in Spain, formerly Physician to Pope Alexander the Sixth, published in the year 1500. "This Disease (says he) was first discovered by the French, when they entered Italy with a strong army, and remained there for some time;

especially while they kept possession of the kingdom of Naples. Wherefore by the Italians it was called the French Disease, because they imagined that it was natural to the French. On the other hand, in France they called it the Neapolitan Disease, imagining that the French had brought it with them from Naples, because it began first to appear at the time when King Charles returned with his men into France: and as such an extraordinary disease had never been seen by any person in our days, therefore it could not be methodically and perfectly cured, by the most skilful, experienced, and oldest of our Physicians."

ANTHONY BONEVENIUS, of Florence, in a book, *de Abditis Rerum Causis*, printed at Florence in the year 1507, speaks thus concerning the Origin of the Venereal Disease: "In the year of Christ 1496, a new kind of disease spread itself, not only over all Italy, but almost over all Europe. Beginning first in Spain, it travelled thence through Italy, and so over France and other countries, seizing great numbers of people wherever it went."

WENDELINUS HOCK DE BRACKENAW, of the University of Bologne, Professor of Arts and Physick, in 1502, writes thus: "From the year 1494 to the present year 1502, during which time a contagious disease, which is called French, has raged very much. Of this disease, or its efficient, formal, and material cause, neither Ancients nor Moderns have given any certain account, nor have they said nor

wrote any thing satisfactory, neither of its name, nor of its nature or quality:—and it is not without cause, that even the learned men of the Faculty fly from this cruel disease, confessing themselves entirely ignorant of it.”

JACOBUS CATANEUS, a Genoese, in a Treatise, de Morbo Gallico, written about the year 1505, thus relates the history of the first appearance of the Venereal Disease: “In the year 1494 of the Christian Æra, when Charles the Eighth, King of France, invaded the kingdom of Naples, during the Pontificate of Alexander the Fourth, there first appeared in Italy a monstrous disease, which was never seen in any former age, and quite unknown in any part of the world.”

PETER TRAPOLINUS, of Padua, Professor of Philosophy and Physick, who died in the year 1506, published de Morbo Gallico, and says that “this disease was certainly unknown to Hippocrates, Galen, Avicenna, and the rest of the ancient Physicians.”

JOHN DE VIGO, a Genoese, Physician and Surgeon to Pope Julius the Second, in his Practicâ Chirurgicâ Curiosâ, about which he was employed from the year 1503 to the year 1513, and was first published in 1514, expresses himself thus:—“In the month of December, in the year 1494, in which year Charles King of France marched into Italy with a very numerous army, in order to regain the kingdom of Naples, there broke out and spread itself almost all over Italy, a certain disease of an

unknown nature, which different nations have called by different names. It was necessary, in order to cure it, to call in new helps, and to make trial of new medicines: and indeed whatever was found to be successful in that disease, was rather owing to new experiments, than to any assistance from remedies recommended both from reason and experience of the Ancients in their Writings."

PETER MAYNARD, of Verona, about the year 1518, wrote two Treatises de Morbo Gallico—in the former he speaks thus:—"At this present time there has been discovered an epidemical or fatal distemper:—it is called French, because there is no remembrance of it before Charles King of France marched with his army into Italy, viz. about the year 1496."

ULRICH HUTTEN, a German knight, having been infected with the Venereal Disease, and tried the common method of cure to no purpose, no less than eleven times in the space of nine years, was at last restored to perfect health, by the use of Guaicum, about the year 1519, and the same year wrote a learned Treatise de Curatione Morbi Gallici, per Administrationem Lign. Guaic. which he dedicated to the Archbishop and Elector of Mentz and Magdeburg:—He expresses himself thus: "It has pleased God to send diseases upon us *unknown* to our ancestors, as far as we can judge. In the year 1493, or thereabout, there broke out a contagious disease, not in France, but in Naples, though it was called French, because it appeared first in the

French army, which Charles King of France marched into the kingdom of Naples. Besides, the Physicians shunned the very sight of it, much more the touch of the persons who were infected with it, being more afraid of it than any other disease."

JAMES à BOTHINCOURT, of Roan, Doctor of Arts and Physick, published a book, entitled *Purgatorium in Morbum Gallicum sive Venereum*, in which he affirms, "that if this disease afflicted mankind formerly (as some Physicians seem vainly to imagine) there is not the least mention made of it in their Writings. It is certain (proceeds he) that it only appeared amongst us about thirty years ago; which æra of the disease, subtracting thirty years or so, from 1527, will make it happen about 1496."

LAURENCE PHRISIUS, a German, in a *Treatise de Morbo Gallico*, published at Basil, 1532, writes thus:—"In the year 1496, there raged a violent disease, making dreadful havoc upon mankind with ulcers, swellings of the glands, and excruciating pains, and so excessively tormenting, that all who have ever beheld it, have been quite astonished at it; for that destructive disease was *unknown* and *unseen* before, not only by the vulgar, but likewise by men of learning and well skilled in Physick."

PETER ANDREW MATTHIOLUS, of Sienna, in a *Discourse de Morbo Gallico*, written about the year 1535, expressly asserts, "I can assure you, that before the arrival of Charles King of France, Italy

neither knew the disease, nor had ever heard the name of it."

ALPHONSUS FERRUS, a Neapolitan Doctor of Arts and Physick, and Physician to Pope Paul the Third, in a Treatise de Morbo Gallico et Ligni Sancti Naturâ, published at Rence, 1537, expresses himself as follows :—" I do not find that any of the ancient Writers have at all treated in particular of that disease which is called the French. Latin authors have imagined that some of the Antients have hinted at it in general ; but this is only conjecture, and far from being certain. Let your young Authors torment themselves as much as they please, in interpreting the signification of the names and the force of the words, and put that construction upon it, which is most agreeable to themselves, yet none of them will be able to shew a particular chapter upon that subject."

JEROME FRACASTORIUS, a Veronese, de Morbis Contagiosis, published at Venice, in 1546, has these observations on the Venereal Disease :—" Among other wonderful phenomena which have happened in this age, we have seen a disease spring up quite new and unknown in this part of the world, which has overspread almost all Europe, and likewise a great part of Asia and Africa, but in Italy it broke out at the time when the French took possession of the kingdom of Naples, under their King Charles."

ANTHONY MUSA BRASSAVOLUS, of Farrara, in the beginning of his book de Morbo Gallico, which was

printed at Venice, 1553, says, "that the conclusion of the whole affair is this, that the Venereal Disease then took its rise, when the French invested Italy, and Charles the Eighth of France subdued the kingdom of Naples."

GABRIEL FALLOPIUS, a native of Modena, in a discourse de Morbo Gallico, which he wrote in the year 1560, having produced in the first chapter several instances of diseases lately known in Europe, proceeds thus:—"The latest of them is that which we now treat of, and is called the French Disease, or the French Scab; nor is there any question to be made of its novelty, because in the times of our Ancestors it was not yet discovered, it was not yet heard of; in these days the Elephantiasis was the reigning disease. The Scab first attacked us in the year 1494, when Charles, a warrior of great authority, and a very fiery genius, indulging his natural inclination to war, invaded Italy with a large army, which he had gathered together that year; having subdued the dukedom of Milan, and all Tuscany, the Republic of Florence, &c. yielding likewise to his arms, he attacked Rome. From thence he marched to Naples, which he attempted to attack with a very large army of eighty thousand men. It was during this juncture that the disease appeared."

Besides the testimonies I have here produced, if there were any occasion for further proofs that the Venereal Disease first of all appeared at the æra that

I am assigning to it, many more Authors may be brought to corroborate the fact, if a case, in my opinion, already so plain demanded it.

LEONARD SCHMAI, of Saltzburg, de Morbo Gallico ; NICHOLAS MASSA, a Venetian, de Morbo Gallico ; ANTHONY GALLUS ALOYSIUS LABERA, of Avilra in Spain, Physician to the Emperor Charles the Fifth ; JOHN BAPTIST MONTANUS, a Veronese, Tract. de Morbo Gallico ; PROSPER BORGANTIUS of Padua ; NICOLAS MACCHELLUS, of Modena ; ALEX. TRAJAN PETRONIUS ; and lastly, WILLIAM RONDELETIUS, Regius Professor and Chancellor of the University of Montpelier, in the beginning of his book de Morbo Italico.

All these wrote between the years 1500 and 1566, and all of them declare with one consent, that Lues Venerea was a new distemper in Europe, and when it first appeared towards the close of the fifteenth century, was entirely unknown to the Physicians of that day.

CHAPTER III.

HISTORICAL TESTIMONIES.

Of so great importance to society was this disease, and so much concern did it excite among men, that we find the Historians of the time were no more silent upon it than those of other times were of the Sweating Sickness, of the Plague, or of the Fire of London. Historians always mark events which do happen ; and sometimes perhaps they either aggravate or diminish the circumstances of them ; but I believe it will never be thought that they write on diseases, and describe them as existing, if they are not at the time in existence ; nor do they interrupt their more general object by the relation of events which are not less remarkable by being uncommon.

MARK ANTHONY COCCIUS SABELLICUS died at the age of seventy, in the year 1506, of the French Disease, which he had contracted by venery. This Historian, in his *Rhapsodiæ Histor. lib. 9*, first published at Venice in 1502, and afterwards at Paris in 1509, writes thus : “ In the year 1496 a new kind of disease began to spread itself all over Italy, which took its rise the year before, at the time that the French first entered that country, and therefore, as it was natural to believe, was called French ; from

whence that horrid disease, which no age was ever afflicted with before, took its origin, is, in my opinion, far from being certain. After various tortures, there appeared pustules upon the body, which, degenerating into foul ulcers, made it loathsome beyond expression. Neither the Elephantiasis nor Leprosy can be compared with this disease ; and the most melancholy circumstance of all was, that it was prolonged for several years, and the persons infected with it neither died nor recovered. Few died, in proportion to the number of those who were seized with it ; but much fewer escaped it : and not only Italy, but likewise Germany, Slavonia, and the whole coast of Macedonia and Greece were smitten with this dreadful scourge."

BAPTIST FULGOSIUS, a famous Genoese General, published, in the year 1509, nine books of Memorable Actions, in his native language: it was turned into Latin by CAMILLUS GILLINUS, of Milan, and published there. FULGOSIUS relates, that "Two years before Charles came into Italy, a new disease was sent down upon mankind, for which the Physicians could neither find a name nor remedies among the Ancients, and was differently called, according to the different countries which it visited. It greatly affected the joints, and in some the whole body was all over ulcerated. But the most surprising circumstance (adds he) in this disease was, that it exerted its contagious quality only in coition, and always derived its origin from the Genitals. Another re-

markable circumstance attending it was this, that they who were under cure, unless they took particular care to abstain from venery, even after the disease seemed quite gone, relapsed into it, and were as bad as ever. In old persons it appeared to be quite incurable. This Plague (for such indeed it seemed to be) was brought from Spain to Italy, but was communicated to the Spaniards from Æthiopia (the West Indies) and soon spread itself all over the globe."

In 1529, JEAN DE BOURDIGNE', a French Historian, in his *History of the Province of Anjou*, says, "I must not forget to mention that, in the same year (1495) a disease began to rage in France, which the French called the Great Pox, and the Neapolitan Scab, because they were first seized with it in the Neapolitan expedition; and the Italians called it the French Disease, because it appeared first in that country at the time when the French made that irruption into it. It is a dangerous disease, and at first was as bad as the Leprosy; but in process of time its rage is somewhat mitigated, and it is neither so violent nor so contagious as it was formerly. It is certain, however, that it has killed great numbers of people, and many persons of distinction, who for a long time could find no remedy against it. But in the islands lately discovered by the Spaniards, a little while ago was found out a tree, commonly called Guaicum, which is a sovereign and infallible remedy against this disease."

FRANCIS GUICCIARDINO, of Florence, who wrote the History of Italy, of his own time, from the year 1494 to the year 1532, in the Second Book of his History, expresses himself thus, upon the Venereal Disease: "The French being seized with that disease while they were in the possession of Naples, in returning to their native country, spread it all over Italy. And indeed this disease, whether it was altogether new, or was only hitherto unknown, except in some remote corner of the globe, was so very terrible for a great many years, that it seemed the greatest calamity which in the memory of man was to be transmitted to posterity: for it broke out either in nasty pustules, which frequently turned into ulcers of so malignant a nature that it was impossible to cure them, — or discovered itself in most exquisite pains, racking all the joints and nerves of the body. Neither were the remedies used by the Physicians found effectual against it, but frequently quite the contrary, and such as only irritated it. Great numbers it killed, sparing neither age nor sex;—still greater it left entirely disfigured, lame, and subject to perpetual tormenting pains. Nay, a great many of those who appeared to have been cured, in a short time relapsed into the same miserable condition in which they had been before. But after some years, whether from the direful influence of the stars, from which it had its origin, being at last mitigated, or from proper remedies having been found out by long experience, it began

to grow more mild, and was divided into more kinds, and put on different appearances to what it did at first. It is agreed upon by all who have enquired carefully into its nature, that it seldom or never has been communicated to any person except by coition."

It appears from History, that the New World was first discovered by Christopher Columbus, under the patronage and protection of Isabel Queen of Castile and Leon. In August, 1492, he first set sail with three ships and 120 men, arrived at Hispaniola in the month of December of the same year, and returned from thence in March 1493. On the 25th of September following, he set out again from Cadiz with seventeen ships and 1500 men, besides mariners and workmen, and arrived in November again at Hispaniola: from whence in the year following, 1494, he dispatched fourteen ships to Spain. In April, 1494, Barth. Columbus, the brother of Christopher, arrived in Hispaniola with three ships, which returned towards the end of the same year into Spain with Pedro de Margarit, a Catalonian gentleman, and FATHER BAYL, a Benedictine Monk of Catalonia: the former was at that time severely afflicted with the Venereal Disease. In August, 1494, four other ships from Spain arrived at the Island of Hispaniola, under the command of ANTONIO DE TORREZ, which returned in the same time that the others did. Lastly, in October 1495, JOHN AQUADO, the Envoy of their Catholic Majesties, came into Hispaniola, with four ships to enquire

into the crimes laid to Christopher Columbus's charge, and departed for Cadiz the year after, and arrived with Christopher on the 11th of June, 1496, with 200 soldiers infected with the Venereal Disease.

The controversy will be properly said to be at an end, when once we come to this point, as it evidently appears from the testimonies both of Physicians and Historians, who lived at the first appearance of this distemper in Europe, not only that the Venereal Disease was a common distemper in Hispaniola and the neighbouring islands; but that it was thence transported into Spain, on board some of their ships; and that a remedy was sought for it from the same, or some of the neighbouring islands, from whence it sprung, and was pointed out by some of the natives, when no medicine of sufficient efficacy could be found for it in Europe.

Amongst the Physicians who are witnesses to these facts, the chief are ANTHONY MUSA BRASSAVOLUS, JOHN BAPTISTA MONTANUS, GABRIEL FALLOPIUS, and RODERIC DIAZ. These confirm the disease to have originated in the West Indies, and to have been brought over by Columbus's men — That it appeared in Spain first in the year 1493; at Barcelona, and there spread immediately over the whole city; but as it was hitherto unknown, and appeared to be formidable, fasts and religious devotions were appointed to appease the destructful progress of the disease — That in the West Indies the

disease was very frequent, and familiar to the natives — That amongst them they had found an antidote against it, which was called the Guaicum Wood — That there it was milder than in Europe, where, when it was transplanted, it became so fierce as to affect the head, eyes, nose, palate, skin, flesh, bones, and ligaments. But the Historians of the time appear to me to be more unexceptionable, and represent the first origin of the Venereal Disease, as a fact they were well acquainted with, and of which they were eye-witnesses.

GONSALVO FERNANDEZ DE OVIEDO, who was sent into Hispaniola by King Ferdinand, in 1518, to inspect the melting of metals, and made a long stay in that country, about the year 1535 wrote the Natural and General History of the Indies. In this History he relates, “ 1st. That this disease, which is called de las Buas, is common to all those countries, and therefore it had pleased the Divine Providence to communicate assistance to them all, and to furnish them every where with a proper remedy for curing it. But although,” continues he, “ that disease is frequent elsewhere, yet it is chiefly known amongst the Christians, and cured by means of a tree called Guaicum in this Island of Hispaniola. Amongst the Indians it is not so severe, nor so dangerous, as it is in Spain and the colder climates, for the natives are cured by means of this tree. In these Indian countries there are very few Christians who lie with the female natives, that escape this terrible

disease : because it is really the produce of this soil, and as common to these Indians, as other distempers are in other countries." In the Summary of the Natural and General History of the West Indies, which, upon his returning to Europe from Hispaniola, where he had lived twelve years, the same Author wrote in Spanish at Toledo, in 1525, he thus addressed himself to Charles the Fifth, King of Spain, by whose direction he complied :— I can assure your Imperial Majesty, that this disease, which is new in Europe, is very well known in the Antille Islands, lately discovered, and so very common there, that almost every one of the Spaniards who lay with the Indian women contracted it from them. Thus it was imported from thence into Spain, by those who returned with Columbus after his first and second voyage. And afterwards, in the year 1495, when GONSALVO FERNANDEZ of CORDUBA, at that time a famous General, marched with the Spanish forces into Italy, by order of their Catholic Majesties, Ferdinand and Isabella, to the assistance of Ferdinand the Second, King of Naples, against Charles King of France, there were several of the Spanish soldiers infected with the same disease, and thus it was quickly communicated with the Neapolitans and the French, by means of their lying promiscuously with the same women."

To the like purpose the same Author writes in his Natural and General History of the West Indies, in book the second, which is a much larger work than

his Summary, and written in Spanish ten years after, viz. in 1535. There he expressly names several witnesses of éminence, who beheld what passed, and were concerned in the first expedition made into Hispaniola, and from whom he declares he received whatever he relates. And more especially he relies upon the authority and testimony of Pedro de Margarit, the Catalonian, whose testimony is of the greater value, as he attended on Christopher Columbus in his second expedition into Hispaniola in 1493, and returned from thence in the year following, infected with the Venereal Disease, of which the Author suspected he was not perfectly cured at the time he wrote; because he was even at that time tortured with continual pains. Upon a thorough knowledge, therefore, of the fact, this Author expressly declares, that the Venereal Disease was brought from Hispaniola into Spain previously to the year 1495, by the Spanish soldiers, who served under Gonsalvo Fernandez in Italy, and communicated to the French and Neopolitans by promiscuous venery.

FRANCIS GUICCIARDINO, Citizen of Florence, who in the History of his own Times, written in Italy, has carefully related whatever passed in Italy from the years 1494 to 1532, at the close of his second book, having observed that the Venereal Disease was commonly called the French Disease, he thus goes on: "But it is no more than just to remove this reproach from the French. For it is certain

that the Venereal Disease, together with a most inveterate itch, was imported from Spain to Naples. Neither, indeed, was it a native of that country, but was likewise imported thither from those islands, which about that time were discovered by Christopher Columbus of Genoa. But in those islands kind Nature has provided a speedy remedy against this disease, viz. only by drinking the juice of an excellent tree that grows there, endowed with many other properties, they are very easily cured. This tree is called Guaicum."

FRANCIS LOPEZ, of Goinara, a Sevillian Clergyman, and Chaplain to Ferdinand Cortez, who reduced the Empire of Mexico into a Spanish Province, in his General History of the Indies, written originally in Spanish, and published at Medino del Campo in the year 1553, gives us the following account: "All the natives of the Island of Hispaniola are infected with the Venereal Disease, and therefore the Spaniards who had to do with the Indian women very soon were seized with this complaint, than which there is none more infectious, or more painful. Wherefore, finding themselves very much tormented, and in no way of getting better, they returned into Spain, most part of them on account of their health, and some on account of business. The disease still lurking in those who returned, was communicated by them to a great many common women, and by these to several of the soldiers who

marched into Italy, to fight under King Ferdinand the Second against the French.

“ Thus the Venereal Disease was introduced into Italy; and as it broke out amongst the French, upon their first entering into that country, it was therefore by them called the Neapolitan Disease, because they imagined they contracted it from the Neapolitans: on the other hand, by the Italians it was called the French Disease, because they fancied that it was brought to them by French: though there were some who likewise called it the Spanish Scab. Further, as the disease, so likewise the remedy for it came from the West Indies; a very strong argument that the disease came originally from thence. That remedy is a kind of wood or tree called Guaiacum, with which the high mountains in that country are covered.”

FATHER JOHN BAPTIST DU TERTRE, of the Order of Preaching Friars, in his General History of the Islands of St. Christopher, Guadaloupe, Martinico, and others, says, that “ That foul distemper which they call Epan, and is really the Venereal Disease, is, in a manner, hereditary to the natives of those islands, who not only contract it by venery, but it likewise breaks out spontaneously upon them, owing to their bad manner of living, and the unwholesome food which they eat. He then adds, that “ to his certain knowledge there were Spanish soldiers who, upon their return to Spain, the first voyage with Christopher Columbus, contracted this disease from

the natives, and carried it with them to Naples, where the infection was communicated to the French, and hence it was spread over Europe.

To the same purpose, and with equal strength from authorities, speak PAUL JULIUS, a Student in Physic, who was advanced to the Bishopric of Nucerias by Clement the Seventh, and died in 1552 — John Mariana, a Jesuit, a very careful collector of Spanish History — and Frances Eades de Mezaray, in *Abbrégé Chronologique de l'Histoire de France*, in the year 1496. But it will be certainly thought unnecessary to quote their words at large, in a case so plain as I think I have made it out.

In the Neapolitan, or rather in the Spanish Army, there were not a few of the soldiers who, returning from the Indies, either in the first voyage with Christopher Columbus, in the month of March, 1493; or in greater numbers, in the second, as already observed, with Antonio de Torrez, in the beginning of the year 1494; or in the third, with Pedro de Margarit, at the end of the same year, were as yet infected with the Venereal Disease, or at least had contracted it in Spain, after it had been brought by others into Europe. And therefore it is by no means strange, that many of the Neapolitans should be infected with the same distemper, as they served under the same colours, and had to do with the same women that followed the camp. And, for the same reason, the like contagion could not but soon be communicated by one or both of them to the French,

for, as the success of the war continued doubtful for two whole years, and the same towns were taken and retaken by both parties, it is plain that the French also must have had communication with the same women who had lain with the Spaniards and Neapolitans, and thus the seeds of the Venereal Disease must have mutually passed from one to another.

But though this disease appears to have spread itself very quickly from Spain, Italy, and France, where it first began, into the countries of Europe, yet its progress was gradual, and slower in proportion to the distance of those countries from the places where it first shewed itself. Of this we have the testimony of DANIEL SENNERTUS, about the year 1632, concerning the Upper Saxony, who, in the sixth book of his Works, after relating that Cappavacius, in Italy, had got above eighteen thousand crowns, solely by curing of the French Disease, ingenuously owns, that, "for thirty-four years, during which space he had practised physick, not without profit, he had not made so many single crowns by curing this disease, as Cappavacius had made thousands, owing to the small number of those who laboured under it in that country."

However, at the same time that the infection of the Venereal Disease passed from Spain into Italy and France, and from thence into the other European countries, it appears likewise to have spread itself along the coasts of Africa and Asia, that were

adjacent to the Mediterranean Sea ; which we think was owing to the following causes : to the banishment of the Jews and Mahometans, who, upon the conquest of Granada, were driven out of Spain by Ferdinand and Isabella, and such of them who fled into Africa carried with them the Venereal Disease, which they had before contracted in Spain.

Of this we have a remarkable witness, JOHN LEO, a Mahometan, and a native of Granada, who, being banished with the rest, removed to Fez, in Morocco, where he applied himself to the study of the Arabian learning, and being afterwards taken by Pirates, and presented to Pope Leo the Tenth, embraced the Christian Religion. Now in his Description of Africa, which he wrote in Arabic, at Rome, and which was turned into Latin by John Florianus, he expresses himself thus : “ Even the name of this disease was not known to the Africans, before King Ferdinand banished all the Jews and Mahometans out of Spain, who being returned to their native country, the Æthiopians lay with their wives, and thus the disease spread over Africa, like a Plague, so that there was hardly a family that remained free from the infection. They are firmly persuaded that it was transplanted to them from Spain, and on this very account (that it might not be without a name) they called it the Spanish Disease : but at Tunis, and all over Italy, it is called the French Disease ; and the same name is given to it in Egypt and

Syria;—whence the common proverb of cursing—may the French Disease kill you.”

The Merchants and seamen, who brought goods daily from the ports of Italy, France, and Spain, to the ports of Africa and Asia, carried over also a very bad kind of merchandise, viz. the Venereal Disease, which, by degrees, spread itself farther into the country, from the coasts bordering upon the Mediterranean. Nor did the contagion, spread by the Europeans, stop here, but extended wide into the remotest corners of Asia; and, by repeated voyages, penetrated into the East-Indies, where, by the Persians, &c. it is called the Disease of the Christians. It is also certain that the first seeds of this disease were imported by the Portuguese, not only to the coasts of India and the Empire of China, but likewise to the Kingdom of Japan; for ENGELBERT KEMPFER, in his Natural, Civil, and Ecclesiastical History of Japan, says, that “the Venereal Disease, which is common amongst them, is called by the Japanese, the Portuguese Disease.”

Without the idea be well inculcated, and continually impressed upon the mind, that every separate symptom belonging to Lues Venerea is to be found through other diseases, we should hesitate, and be left in some uncertainty from this cause.

There cannot be a doubt that every symptom singly of Lues Venerea, may be a resembling symptom of another disease; but there is no other disease which embraces all the symptoms of Lues Venerea.

There is no other disease where the symptoms of it go progressively on in resemblance to Lues Venerea.

From VIGO, FALLOPIUS MASSA, and BERENGARIUS CARPUS, we can now know what are the true symptoms of Lues Venerea, so strongly do their definitions correspond with the present appearances of it.

It is not incumbent upon me to say how the Lepra has passed away, and is now scarcely heard of: I am only to assert that the symptoms of the Lepra are described to be different from those of Lues Venerea; and that the Physicians, in the beginning of the sixteenth century were perfectly acquainted with the Lepra, but were at a loss to account for, or define Lues Venerea. This was a period when their similarity might have been compared with some pretensions to truth, when the comparison might have been made with success, by the two diseases being brought face to face, if there had been any suspicion then, that their strong resemblance afforded a probability of their being the same.

The possibility of the disease being brought from America to Europe, has been formerly also denied, but we have seen in our time, that it can be conveyed in a longer voyage; for it has been imported to OTAHEITE' from Europe. There we find the disease committing dreadful havoc, proceeding through

all its stages in the fullest extent, from not being disguised or restrained by mercury.

It remains at present, and probably it ever will be a truth unascertained, whether BOUGANVILLE, the French circumnavigator, or WALLIS, first introduced through their crew Lues Venerea to the unfortunate inhabitants of OTAHEITE'. As the act reflects no glory, as there is nothing in the deed which can be honourably boasted of, I presume neither of them have been industrious to assist the Historian in discovering or fixing the fact. If a prize had been the bone of contention, the claim would have been long ago made out, and the right adjudged. If the anxious Historian seeks to ascertain how the disease found a way to OTAHEITE', a repugnance to that which must be ever considered as an unpardonable neglect, and an indelible disgrace, will stifle the enquiry. For we have found, the more the question has been agitated, the more has art been practised to darken it.

The British navigators must have landed at OTAHEITE', because the trophies of their visit are deposited in the British Museum; and they or the French, for it is not yet decided which, either or both, in the foul spirit of the passions, have disgraced, even in the eye of a savage people, the honor of civilized nations, by the selfish gratification of baneful lust.

The only account which we are furnished with is, that the disease was actually there since the Euro-

pean philosophic Captains touched upon the Island; that CAPTAIN COOK found it there, but he made no mention of any particular symptoms, such as Gonorrhœa, Chancre, or Bubo. He presumed "they had found out a specifick for it," although in that I believe he was mistaken, as will hereafter appear. "He had seen some upon whom the disease had made a rapid progress, and one who went away infected, returned in a short time in perfect health." In another part of this voyage, he says, "that some had fallen victims to its rage, abandoned by their nearest relations: their hair and their nails fell off, and their flesh rotted from their bones." He says, in his last voyage, "they had broad ulcers, with thick white edges that were shocking to look at; and yet he met with some that seemed to be cured of it, and others in a fair way of being cured; but this was not effected without the loss of the nose, or the best part of it." Afterwards he tells us, "that the disease is now almost universal: for this they seem to have no remedy. They allow that in a few cases, Nature, without the assistance of a Physician, exterminates the poison, and perfect recovery is produced; but they also say, if a man is infected with it, he will communicate it to others in the same house, by feeding out of the same utensils, or handling them."

It would have been more accordant with the distinct provinces of men, if CAPTAIN COOK had delegated to his Surgeon the office of describing the

symptoms and the progress of the disease. We then should have been enabled to have comprehended the account of it in a more minute and professional manner. An ignorance of the nature of the disease could not be now pleaded in apology for such an omission, with the same pretence as it might have been by COLUMBUS; and therefore it was a duty due to a country, that a more clear information should have been given. But imperfect as they are, these accounts by CAPTAIN COOK, of the progress of LUES VENEREA, correspond with the progress of it when it first appeared in Europe.

CHAPTER IV.

ON THE NATURE AND ACTION OF LUES
VENEREA.

LUES VENEREA appears to be a poison, *sui generis*, —a poison peculiar to the human subject. Every poisonous disease is found to be propagated either through morbid effluvia, or through poisonous fluid conveyed from a diseased subject, and lodged with a sound one. The Plague and the Small-Pox are instances of the former description, and which may be termed contagious diseases: Lues Venerea and bites of mad and venomous animals are instances of the latter, and which may be termed infectious diseases. This distinction between these diseases is evident; for although a contagious disease can be propagated like an infectious disease by inoculation; yet an infectious disease cannot be propagated by effluvia, at least it has not yet been proved to our satisfaction that what I have defined to be an infectious disease, is possessed of that power.

There is no disease which ever was defined, so full of variety as Lues Venerea. The reason of this variety will not be found, I flatter myself, too difficult for some additional explanation. In order to account for venereal action on the various parts, both local and constitutional, we must consider the quality of

the parts on which it is acting: we must consider that venereal infection has the singular power of acting on every component part of the human body: and we must further consider, that, independent of a local and constitutional power, which evidently belongs to it, and which is in general acknowledged, there is a relative distinction to be explained, by which the true nature and action of it will be better understood. Although in this country we have no instance to establish an opinion that *Lues Venerea* can be propagated in any other way than by contact of venereal fluid, conveyed from a diseased subject to a sound one, yet, if its nature does not permit it to be propagated by effluvia, the subtilty of the poisonous fluid, when once it has taken possession, cannot be disputed. It is owing to the uncommon subtilty of venereal poison, and the power which it has of attacking every component part of the body, to the irregular action of the poison on each part in point of succession, to the length of time it will remain without discovering itself, to the progress of it not being definable under any fixed periods, to its assimilation on various parts to other diseases which such parts are liable to, that so many disputations have engaged authors, so many erroneous opinions have been given, such various theories have been adopted, such contradictory practice in the treatment of it has been suggested, such false conclusions drawn, and such impositions on credulity practised.

As the Ancients knew no disease like it, and were not aware that it could be transplanted from the West-Indies, they assigned the origin of it to preternatural causes, not being able to comprehend a natural one. I can always conceive the errors arising from ignorance to be venial, when I cannot with so much readiness find in my duty an apology for scepticism. Modern Theorists, who do not believe that this disease was first brought from the West Indies, or that it could be, might as well doubt that it has since been conveyed from Europe to Otaheité. And those who doubt that a virulent Gonorrhœa is the venereal poison acting locally on a mucous membrane, might also doubt that it is produced, either in consequence of a connection between a sound subject and a diseased one, or in consequence of the infecting fluid being conveyed from a diseased subject, and lodged on a mucous surface of a sound subject, so as to take effect. If we were to make a surrender of our own reasoning upon this question, and build our faith upon the assertions of some, we should become the slaves of constant deception. Gonorrhœa and Chancre are both the result of venereal poison acting on parts, under different modifications. The cause of both symptoms is the same, and the effects will be corresponding to the anatomical nature of both parts: venereal fluid applied to the Urethra, produces a discharge of Mucus; that fluid lodged on the cutis, produces a Chancre.

The Urethra, like every other mucous membrane of the body—like that of the nose, the fauces, and the eyes, is by Nature so formed as to yield from its surface constant moisture, and the surface which secretes this constant moisture is more immediately susceptible than a part covered with skin. That which will irritate the membranes of the nose and fauces, the coats of the eyes, and the Urethra, will not produce the smallest effect on the common skin.—Like a spark of fire, which falling on gun-powder will produce an explosion; but when falling on sand, it will there become extinct. Any little innocent extraneous substance fastening on these mucous surfaces, will excite from them an additional discharge, will stimulate them beyond their general action; and it seems to be quite compatible with the animal œconomy, that the particular or preternatural action of every part, is a possession of power enjoyed to indulge or become free of that which is acting upon it, but more especially on parts of this description, which are both inlets and outlets of the constitution; when any thing which is obnoxious stimulates them, such stimulating extraneous qualities are diluted away by an increased secretion; this increased secretion diminishes the acrimony of that which stimulates; and from this cause, an extraneous substance stimulating a mucous membrane, may be carried away in the secretion, and if it be, then all irritation being at an end, the discharge of secretion, in course, ceases.

On whatever part this infecting fluid lodges, if its action take effect, that action will always be the same, for otherwise it would not be uniform venereal action; the symptoms of its action will only vary, according to the anatomical nature of the parts on which the fluid finds a lodgment. The application of venereal fluid to any part of the body, will necessarily produce a symptom conformable to the part on which it is applied.

Independent of the subtilty of the venereal poison being a cause of our want of a clear and decisive knowledge of the various symptoms of the venereal infection, the want of explanation of what we daily see, will always leave a door open for disputation to enter in; and where the room for enquiry is large and dark, every one will bring out of it, what comes first to hand, and producing it to light, flatter himself that he has brought forth the truth. With some, Gonorrhœa has no relation with Lues Venerea; with others, if Gonorrhœa has any relation with Lues Venerea, it is so remote, so comparatively innocent, so free of every thing obnoxious to the constitution, so easy to be cured, or so unnecessary to be cured, that no harm can follow the contracting it, no bad consequences are in the least to be dreaded. We are positively told that Gonorrhœa is not a venereal symptom, or if it be, that it will cure itself, and that it cannot or will not infect the constitution.

When venereal symptoms, such as Chancres, Ulcers, Nodes, and Caries Bones, presented them-

selves before the Ancients, they did not know the true progress of these symptoms at first; they did not know from what original source they sprung, but, *primâ facie*, they saw alarming symptoms — symptoms new to their eyes, and other senses — symptoms which they could not cure, nor account for: they sought their origin in divine judgment, in poisoning wines, in poisoning water, in deluges of rain, and in various other false causes. To their own ignorance and astonishment at these symptoms, they could plead in plain language; and here they speak through their writings perfectly intelligible. They tell us what they saw, but not how that which they saw was produced; they tell us such were the symptoms, such was the havoc of them upon the human constitution, that they were new to them in their nature, and in this, they told us all that was then known by them. But a discharge from the Penis and Vagina at that time was not a new symptom — a discharge from either is perhaps as old as our race; and when it appeared upon a subject then, produced in all probability less alarm and less enquiry than it would now: for previous to the origin of *Lues Venerea*, the Urethra and the Vagina were liable to every disease which they now are, excepting that produced by venereal irritation; and if the symptom of discharge be the same from other irritating causes, as well as venereal, it will be no longer a wonder that they should overlook this at first, as a venereal symptom, and more especially considering,

that as the consequences of the constitution being affected were so remote from such a local symptom, it would have been almost amounting to a miracle if it had at first occurred to them. Besides, they overlooked this symptom as a *prima ordo* of the disease from another unsuspecting cause: they did not know that venereal fluid could be insinuated up the Urethra, and there lay a foundation for infection, which would act, not only on the Urethra, and not only spread its virulent effects there, but would also be continued on and conveyed from one to another.

And as they did not at first annex the symptom through ignorance, so did they afterwards annex it through information. If they had first of all annexed it through ignorance, and afterwards rejected it through information, then the case would have been widely different: for it would have been apparent, they were apprised of the fact from the very beginning, had weighed it, and had decided to reject it. But from the very first time that *Gonorrhœa* was adjudged to be an increased discharge, flowing from the Urethra, in consequence of venereal infection, and was considered as true a symptom on that part as a *Chancre* was on another part, that fact has not, until very lately, been resisted. In the whole of the historical tracts on *Lues Venerea*, which have been written year after year, that fact, so far from being doubted, has been always established.

With respect to the power which there is in the infecting fluid, whilst acting on the Urethra in man, or Vagina in woman, of producing either diseased effects on the parts locally, or of being from them absorbed into the constitution, and of producing thereby confirmed venereal symptoms—this will be a future subject of consideration.

The common way by which the propagation of Lues Venerea is supported and carried on, and has been from the first history of it in Europe, (it is almost needless to say it) is by connection between the sexes. But although the propagation of the disease is continued by this intercourse of the sexes, and although the act of copulation between the sexes is the common medium for transferring the disease from one to another, yet that is not the only manner by which it can be conveyed. Venereal fluid, when conveyed through any medium from an infected subject, and lodged with a sound subject, will, if it acts, as completely infect the sound subject, as if it were contracted through an amorous intercourse.

So complex and difficult I have ever found it to determine, a priori, whether a discharge either from the Urethra or Vagina possesses a power of conveying infection, that as I cannot fix in my mind a general rule to direct me, so I am at a loss to explain it. An opinion upon this question is very often an opinion of serious importance; no less than the future happiness or misery, riches or poverty, virtue or infamy of individuals are found to be depending on it.

✓We even should be cautious in aspersing or fixing a criminality upon many of those who, knowing they have been injured, do convey an injury without any intention, or without any belief that they could, when they did convey it. For if the most observing of us all be deceived with respect to the point of time when the infecting power is extinct, before the discharge which conveys it ceases; if we plead ignorance upon this question, and I am certain that, a priori, we ought and we must, how much more ought women to be excused, or at least ought they to be indulged with a latitude of apology, who are not of the profession to know. An attention to this part of the subject must therefore be fixed; for it is at a period of the disease which we least suspect, when the violence of the symptoms have subsided, and some time after even, that such mistakes have happened, and that such an injury has been produced, by the premature hazard of enjoyment.

I shall now proceed to give opinions, which, however erroneous, I will take care they shall only err on the side of caution and safety. I will not permit a man (as Mr. Hunter does) to have connection, in a state of disease, and bare the woman harmless. I would not carry this opinion so far as he does, and “allow a man who has a Gonorrhœa to have connection with a sound woman, if he took great care to clear all the parts of any matter, by first syringing the Urethra, making water, and washing the glans.”

My reason for this interruption is, that the parts will go on in secretion during the coⁿ action; and that no punishment is already fix^d for me, which can bear any proportion to the injury my theory would do, if I did permit it. Every man who reflects, and who loves truth, knows that the Urethra will yield an increased secretion during the act of copulation, and with many that increased secretion will be discharged, whether the act of emission be gone on with or not.

When a Gonorrhœa has been protracted for six weeks or two months, or beyond that time; when all the inflammatory symptoms have subsided; when nothing but a thick discharge remains, and a subject, thus conditioned, becomes desirous to form a connection, where he must not injure, is it safe? I take upon me to say that it is not safe. This is generally the predicament in which many well-intending women are found; for they, from the nature of the parts, cannot with so much accuracy decide whether any infectious discharge remains or not: they have most of them some discharge when they have never had any infection; and it is in this doubtful situation that, to their confusion, they do sometimes ruin their prospects by so unfortunate a discovery.

A man may have a Gleet, who never has had a Gonorrhœa; and a Fluor Albus may most certainly be found upon a woman, where the foulest suspicion dares not fix a worse idea. A man may marry, with

a Gleet upon him; and a woman, with a Fluor Albus. But it behoves us to beware that the latter end of an infectious discharge be not construed into a Gleet or Fluor Albus; and whilst a discharge remains, after a confirmed Gonorrhœa, on both sexes, it behoves them to get completely rid of it, or at least to remember that unfortunate discoveries have been made.

The two general local first symptoms of Lues Venerea are said, by me, to be Gonorrhœa and Chancre: the former consists of a virulent discharge of fluid from the Urethra in man, and Vagina in woman; the latter may be an ulcer on any part of the Penis of a man, or Labia of a woman; or on any other part of the cutis or cuticle, where the venereal fluid which is necessary to infect, has been so lodged as to produce that effect.

It has been a question of late years, whether the infecting discharge from the Urethra is capable of producing a Chancre on another subject—or whether chancrous discharge is capable of producing a Gonorrhœa on another subject—or whether a subject, having a Chancre, can infect another with a Chancre and Gonorrhœa—or whether a subject, having a Gonorrhœa, can infect another with a Chancre and Gonorrhœa? Whether these two symptoms be produced from virus, the same in its nature, but only acting on two different anatomical parts—or whether the virus which produces them be not of a different nature, so that Gonorrhœa from one can-

not produce Chancre on another; nor can Chancre from one, produce Gonorrhœa on another?

By those who have doubted they were the same, gonorrhœal fluid has been applied to the cuticle and cutis, for the purpose of proving whether Chancre could be produced by it or not; and chancrous fluid has also been applied to mucous parts, for the purpose of proving whether Gonorrhœa could be so produced. When this experiment was made by one person it succeeded, and when it was made by another it failed. A third person, and who was apprised of the two former experiments, still doubts whether the point in question is clear or not, and therefore thinks, that the fact, to be completely established, wants a stronger confirmation. We were not told by them, whether the experiment was made on the same subject, or on others.

All experiments which are made to ascertain whether the poison of Gonorrhœa and Chancre be the same, are artificial means for ascertaining a fact, better to be ascertained by natural events. But it should not be forgotten, that a fact may fail of being proved, through an error in the experiment. In this question the truth could be well ascertained, if one experiment succeeded out of an hundred; for if gonorrhœal fluid could not produce Chancre, nor chancrous fluid produce Gonorrhœa, if that were true, artificial experiments might be made to eternity without prospect of success. But it only remains that one should succeed, and the fact is proved. A

man, for instance, may deny that a single ball discharged from a gun will kill a bird flying, and the trial might be repeated by a very bad shot, at least a thousand times, without success. Therefore, if the experiment which proves that chancrous and gonorrhœal fluid are both the same in their nature, has been once found successful, all contest upon this question must be at an end, if you really give due credit to the person who tried it.

But is it not wonderful that those who with so much curiosity had given us experiments of art to ascertain the sameness of the poison, should have said nothing about what might be observed upon the natural progress of the poison? Or do men expect, when they want to be obliged by Nature, and have their own wishes gratified, that she will yield more information by being tampered with through art, than by being left to her own action, and to be viewed in her proper light in which she presents herself? Have they told us they were induced to make those experiments, because there was no other means of ascertaining the fact? And is that the case? Is it impossible to examine a woman, who from a natural intercourse has infected a man, and from that examination to decide, whether she infected him from Gonorrhœa or from Chancre? Is there a difficulty in discovering a Chancre, if she has one—and if there be no Chancre, must not such infection have been from Gonorrhœa?

If women who infected men with Chancres, all necessarily possessed Chancres, certainly those Chancres would go on increasing to such a degree, that they could not long continue to receive embraces, and would be obliged to keep to themselves, at least till their Chancres were healed. One might expressly declare, without being deep in venereal knowledge, that women so conditioned would be in retirement for a little while; for if they continued to appear in company, whilst they took mercury, their breath would betray them, and their intentions would be defeated. But in Gonorrhœa, the case is widely different. Many men who have been infected with Chancre, by women who had only Gonorrhœa, have been obliged to abandon their charges, against them, after the women have submitted to examination, and their avowed innocence has been confirmed, by their never being out of the way, nor by the least change of their health: but a Chancre, which is important enough to infect, never can be concealed, whenever it be looked for. But there is a new observation which I have to make, and which is, that the effect of infectious secretions from one subject, acting upon another, differs essentially from the infectious secretions of a subject taken from one part and applied to another part of the same subject. Those who have been making the experiments I have alluded to, knew nothing of this distinction.

The fact, that the fluid of Gonorrhœa and the fluid of Chancre are both the same, I have no doubt about.

One reason, and I think, upon the face of it, the most plausible of all others, why the fluid of Chancre and Gonorrhœa may be said to be not the same, is that a man may have a Gonorrhœa without a Chancre, and a Chancre or Chancres without a Gonorrhœa. And it might be also held that this is in general the case, and also that the one does not produce the other upon the same subject, and that the one is not commonly found, when the other is present. It is upon this point, I think, the advocates for the opinion, that the fluid of Gonorrhœa and Chancre are not the same, may make a stronger stand, than at any other. Is it not strange (they may say) that, if both the fluids possess the same virus, Chancres do not inevitably accompany Gonorrhœa, and Gonorrhœa Chancres, on the same subject? They might also add, if both infections were not received at first, yet if they be of the same quality, why does not the continual flowing of Gonorrhœa produce Chancre upon the Glans and Prepuce; and why does not chancrous fluid produce Gonorrhœa?

I have placed this objection in the strongest point of view, because I think it my duty to do it; for I should be sorry to be thought either positive or mean in argument. He who has not the candour to listen to the arguments of others, must be confi-

dent that he cannot retain enough of his own to command their conviction.

I shall answer this objection under two separate heads :

First, what I have to say with respect to the local action of infection, as it is received from another subject ; and secondly, that which offers to me on the local action of infection upon other parts of the same subject, after it is received.

The two first symptoms, which are Gonorrhœa and Chancre, are already confessed ; but when they both are found on the same subject, and from the same contraction, their order of appearing perhaps may not be so generally known. When a person has contracted both symptoms from the same connection, Gonorrhœa will be the first to betray the infection—the Corona Glandis, Frœnum, and the internal part of the Prepuce will be the next—and the cutis, or the external part of the Prepuce, will be the last. I have known the space of time between the appearance of Gonorrhœa and that of Chancre on the cutis of the Penis to be three weeks ; and yet as there was but one connection to produce both, I am confident the one did not arise out of the other ; but the true cause of the distance of periods between them was, that it would take but a short time for the venereal stimulus to produce a discharge from the Urethra, and a longer time for the same stimulus to produce a Chancre on the cutis ; and also, that it would take an intermediate time

between the former and the latter for the same stimulus to produce a Chancre on the glans, internal part of the Prepuce, or Frœnum.

When all these symptoms do appear from one infection, such is the general order of their appearance ; I say general, because I do not suppose but there might be exceptions. It is also very well known to those whose practice affords them the opportunity of obtaining that knowledge, that a Gonorrhœa is the most general first symptom, when it comes on alone ; that Chancres on the Frœnum, and internal part of the Prepuce, are the next most in common ; and that Chancres on the external part of the Penis, and parts adjacent, are the most uncommon. Why this difference is found, it is not owing to the virus from the woman not coming into contact with the one part more than the other, but to the difference in the construction of those various parts—to the more susceptibility of the one than the other—to the one being more disposed to be stimulated by the virus than the other—to the one being capable of being acted upon by the virus when the other may not—to the virus being lodged and remaining undisturbed—to the faint power which the virus might at the time possess when the infection was given—or to the difficulty there is in the subject who is infected, to be infected.

For if a man be connected with a woman who has a Gonorrhœa, every part of the Penis may be presumed to take up the moisture ; but as the cutis

is more difficult to be infected than the cuticle, and the cuticle than the Urethra; and as the fluid lodging on the external part of the Penis is more liable to be wiped away, so does it happen that that part is the most exceptionable. The Urethra being a part liable to be stimulated by a stimulus which would not stimulate the skin, is, of course, liable to be stimulated by a power which in its nature may be too faint to stimulate or act on the Prepuce: besides, the natural formation of the Urethra prompts it to throw off any thing which is obnoxious to it, or at least, it is readily prompted to discover whatever produces an irritation upon it: and therefore also, it might very naturally be inferred, that if there be stages of the Gonorrhœa when the power of the infecting fluid is weaker, and less active, the Urethra, from its nature and construction, will be liable to be acted upon by it, when other parts cannot.

In order that I may be well understood, I shall explain what I mean by the power of the virus being faint at a certain stage of the disease, and how far that faintness or defect of power might operate or affect a person who is infected from virus so conditioned. I mean, if a woman possesses a Chancre or Gonorrhœa, or both, and the fluid of which, at the period when the man cohabits with her, is so conditioned, that the discharge is very kindly, that the Chancre is healing, that the Gonorrhœa is almost gone—I mean that the fluid thus conditioned, when

it does infect, is not capable of infecting the cutis, when it is capable of stimulating the Urethra. I mean to point out, by this observation only, the probability there is that the virus, at such a stage, cannot act on any other part, less irritable than the Urethra, or a surface similar to it. This appears to me to be a reason why a person may have a Gonorrhœa, without a Chancre; and why a Gonorrhœa prevails much more than Chancre does. But I believe it often happens, that the undisturbed lodgment of the virus is the cause of the appearance of the first infection on any part.

Although I have said that a subject may be infected with a Gonorrhœa, who, through the faintness of the virus, could not be infected with a Chancre, yet I do not mean to be understood, that the symptoms of a Gonorrhœa, upon the subject so infected, will be in the least influenced from the greater or less degree of virulence in the infection of the subject who infects: for as the infection is proved to have taken effect, the symptoms will not depend upon the condition of the venereal poison, as it was in another—they will depend upon the nature of the constitution, and upon the degree of irritability of the subject who has received the infection. And thus is this observation, in its nature, correspondent with the more general laws of every other infecting poison. This manner of reasoning will be found universally true, both as to the local action of poisons, as well as to their constitutional

effect. If I were to inoculate a person with the Small-Pox, and took the matter from another who had only a single pustule, it does not follow, but the inoculated subject may sink by the load of the disease. In the Jail Distemper, as it is called, the prisoner who is able to appear in Court to take his trial, may circulate that infection, which has often proved fatal to those who caught it.

The last observation which I have to offer is—the difficulty there is in a subject who is in the way of infection, to be infected—and this is a cause why such a subject may be liable to contract a Gonorrhœa, when he may not a Chancre. This difficulty of receiving the infection may be either natural to the constitution, in the first instance, or be produced from habit. In the natural constitution, if it be in general irritable, parts of it will also partake of that general irritability, and will of course be more susceptible of the action of virus, than where such irritability is not predominant; such a constitution is more exposed to infection, or at least, suffers much more from it, than one of an opposite description. But the common habit of intercourse with women liable to give the infection, will so far take off from the irritability of the parts, that in many instances men have gone on for years, from one woman to another, without the least sign of infection; whilst a stranger to the town, on his very first connection with them, will immediately feel the virulent effects of the embrace. This fact is so indisputable, that when-

ever the argument is started in conversation, it never fails to be remarked upon ; I do not mean by medical men alone, but by all those who have been in the way of proving the fact. And last of all, to prove that the Urethra as well as the whole of the substance of the Penis, is sometimes not in a disposition to be acted upon by venereal virus, notwithstanding the fluid has evidently been lodged there, I shall call to the recollection, that the first sign of infection has been often pointed out by a Bubo in one or both groins.

Having said what I here intended, with respect to the local action of the infection, as it is received from another subject, I shall now come to the second head, and consider that which offers to me on the local action of the infection upon other parts of the same subject, after it is received by him.

It has been said, and very truly said, that if a Gonorrhœa and Chancre be found upon the same subject, they do not in their progress interfere with each other ; that they appear to be so very distinct, and require such a different treatment, that those who at first suspected them to have not originated from the same virus, will yet, from these observations, have their suspicion more strongly confirmed. If a subject has a Gonorrhœa only, I am of opinion that the Gonorrhœa will not produce a Chancre on the external part of the Penis, nor within the Prepuce. I will go farther, I am confident that a Chancre is never produced from a Gonorrhœa of

the same subject ; and I am also confident that a Chancre on the Penis will not produce a Gonorrhœa on the same subject. This may, in the opinion of some, be still widening the difference in the nature of the two fluids, which, according to my theory, ought to be united. I will go farther, and say, that I have seen a Chancre destroy almost the whole of the glans Penis, without bringing on a Gonorrhœa ; and I have seen a Gonorrhœa produce a swelling of the Penis — an excoriation and discharge upon the glans—an abrasion of the whole of the surface within-side of the Prepuce, without there being the least appearance of a Chancre.

But the truth of all this does not alter the question, because it is no proof, but the infection which produced either or both symptoms might still be the same. This certainly is no proof that the two stimuli were not excited from the same virus ; but it is a proof that the two stimuli, when once excited upon parts whose secretions naturally will be different, and upon the same subject, will not interfere with each other.

I can put this question in a much more logical, and in a much stronger light than I have hitherto done — for hitherto it has been only presumed that the virus is not the same, because Gonorrhœa will not produce Chancre, and Chancre will not produce Gonorrhœa on the same subject. I say hitherto it has been given only as presumptive proof. But I will give a positive proof that this difference may

exist, as to their action, and the virus yet be the same. For as I have observed that Chancre is not produced out of Gonorrhœa on the same subject, so do I now observe, that on the same subject one Chancre will not produce another. This is a positive proof that the first infection received has produced a Chancre or Chancres, but that this Chancre or Chancres will not generate, or, at least, do not generate other Chancres on parts which come into contact with them, for if that were the case, the whole of the Penis would, time after time, become the seat of a succession of Chancres, arising out of ab origine ones. But, on the contrary, the original Chancre continues on, increasing in its size, and growing by degrees worse and worse, until, in all probability, without the interposition of practical art, the whole of the Penis would be destroyed by it.

If the virus from a Chancre could produce Chancres on the same subject, then Chancres would be found wherever the fluid touched and lodged on a sound part, and the whole of the glans would become a galaxy of Chancres.

I will state a case for the illustration of my argument. A gentleman that I was attending at the time of my writing this, first of all perceived a Chancre on the Frœnum, and another about the eighth of an inch from the Frœnum. He had discovered both about three days. On the fourth day, after he applied to me, a pimple was discovered on the dor-

sum Penis, which was observed for three days, before the truth of its nature was ascertained. This also proved to be a Chancre, and the late appearance of it after the two former was most assuredly owing to its being on the skin, as the two former were more early, by being on parts not clothed by skin. They were all three contracted at the same time. These three Chancres never produced a fourth. The two first could not possibly produce the third. The third was quite out of the way of the fluids of the other two; besides, it was discovered too soon after for the other two to be produced by them. I cannot take upon me to say whether the woman who infected this gentleman did it through Gonorrhœa or Chancre; but I will give my reason why I think he was infected by Gonorrhœa and not by Chancre—because two of the Chancres were about the Frœnum, and the other at a distance from them on the dorsum Penis. One Chancre could not have produced this infection from the woman—there must have been more—but the fluid from Gonorrhœa could effect it with facility.

I will now rest this argument upon the result of general experience, and which I am confident is the only solid method of reasoning upon this or any other question; for how are we to know what the Venereal Disease is, or what any other disease is, but from the result of its action. And first, it is evident to my senses, from common observation, that the virus of Gonorrhœa and Chancre are one and

the same : — secondly, that a man may be infected with a Goonrrhœa and a Chancre, or with a Gonorrhœa only, or with a Chancre only, by once cohabiting with a woman infected with either : — thirdly, when I find that Chancres appear all at the same time round the glans Penis, as they often do, I am of opinion that the infection which produced them was from Gonorrhœa; or whenever they appear on the Penis, either all at one time, as they will on parts of the same texture, or whenever they appear at stated periods on parts different in their texture — then I am of opinion they were produced from Gonorrhœa : — fourthly, I do not believe that gonorrhœal fluid does ever produce Chancre on the same subject, nor do I believe that Chancre ever produces Chancre on the same subject or Gonorrhœa. This is, so far, my venereal creed; and this is confirmed by the concurring testimony of natural occurrences, as they offer through the first infections of Gonorrhœa and Chancre.

I have gone no farther with my theory in this Chapter than what is barely necessary to explain my ensuing Chapter. To make theory and practice clear to the understanding, I mean that mine shall go hand in hand.

CHAPTER V.

ON GONORRHŒA.

Venereal infection is always conveyed in some substantial form — either by gonorrhœal fluid, or chancrous or ulcerous fluid: it is in these forms imparted and continued from one subject to another, and which is the very support and existence of the disease. This fluid applied to any part of the body, from one subject to another, will act, although with more difficulty on skin than on a mucous surface.

Whatever part of one subject comes into contact with the infectious fluid of another, that part is exposed to be infected; and as from the nature of the disease, the Penis in coitu is the part thus exposed, so is the Penis in general the first local part infected. The infection there shews itself either in the form of Gonorrhœa or Chancre. But it is of Gonorrhœa that I am now about to treat, for I mean to take another opportunity of enlarging my opinions upon the subject of other modes of infection.

It is obvious to our constant feelings, that when any thing is applied of an irritating property to a mucous surface, it increases a discharge of mucus beyond the common degree of secretion; and when

the Urethra is stimulated by venereal infection, the discharge of venereal mucus is then called a Gonorrhœa.

I am not fond of changing the names of things after they have been long understood to convey certain senses, although they may not critically convey the true idea of that which they are meant to convey ; for a word can only convey the name of a thing, but never the full meaning or description of it ; and therefore if we do but know the thing by a name, that is all which is wanted. I consequently shall continue to call the increased discharge of mucus from the Urethra, when brought on by venereal infection, a virulent Gonorrhœa, although the semen has nothing to do with it, because the disease is already understood by that name, and not so well by any other.

The application of venereal fluid to the Urethra produces a stimulus on the surface of it, and that stimulus must produce a discharge, and so it would if it were applied to any other mucons surface of the body. It is a consequence which must follow, and the symptom of discharge is in this instance the natural effect of the venereal stimulus on the part.

It is not uncommon for men, from their admiration of novelty, to detect one error and to plunge into another — to know what a thing is not, before they know what it is — to be able to convince us from certain reasons, that the thing cannot possibly be what it is asserted to be, and yet to be still at a

loss to say what it really is — or at least, he who says with truth what a thing is not, does not by that establish what it really is. The Ancients called this discharge brought on by the application of venereal virus, a flowing of semen — the Moderns have clearly disapproved it, and we are now convinced that it is not a flowing of the semen. But, although we all now disavow the discharge from the Urethra in a virulent Gonorrhœa to be a flowing of the semen, yet there is a difference of opinion amongst the Moderns about the true nature of this discharge: some will have it to be an increase of mucus from irritation, and others to be pus — to be the true conditioned pus — whilst others take the middle path, denying that it is mucus, and only asserting that it is resembling to pus.

In giving my opinion upon the nature of this discharge, as it is always seen in a virulent Gonorrhœa, I mean to do justice to my own understanding, and to abide by such truths as I can and do see, instead of looking after and producing for evidence arguments which are hidden, and proofs which are obscure. No chemical processes can prove pus and mucus to be the same, if they really be not so — nor can any other mode of experiment, be it either the result of ingenious inquiry, or singular vanity — for Essays upon this subject which have treated it in all its ways have been published, and them I have read.

• If from any irritating cause, the mucus, which is

naturally secreted, and is spread over the surface of the Urethra, be increased—if the glands, which secrete this mucus, from the same cause be also irritated—if there be an increased action in the secreting power of the glands—if the mucus, from that increased action, be not so perfectly elaborated as to assume the appearance of true conditioned mucus, in a healthful state—but if from that increased action it be so elaborated as to assume the true condition of such mucus as constantly flows from glands thus irritated—I shall certainly call such a discharge increased mucus from irritation. It has as great a claim to, and it might with as much propriety retain the appellation of mucus, in this state, as saliva is still called saliva, when the effects of mercury has stimulated the glands of the mouth, and thereby increased the quantity, and changed the taste, smell, and appearance of that fluid.

If the discharge from a virulent Gonorrhœa be not increased mucus, because its colour, smell, and every other appearance vary from mucus which is secreted without irritation, it does not follow, that it should be said to be pus; for this would be admitting that because a thing is so changed, and cannot be believed to be the same thing that it was, it must now become a thing which it cannot be.

The whole of the membrane which covers the fauces, larynx, and internal parts of the nose, the antra of the jaws, the sinuses of the sphenoid and ethmoid bones, and the sacculus lachrymalis, yield

a mucus, limpid in colour, small in quantity, and almost without any smell; but this fluid, either by being confined beyond the necessary time, or by inflammation or irritation of the gland or membrane which secretes or contains it, or even from general affection of the habit, will put on a yellow purulent colour, where there is neither abscess nor ulcer in the part from whence it flows. So many instances of this are producible, as to put the matter beyond a doubt. The lining of all these parts is constantly embued with a mucus, naturally clear, and no more in quantity than is necessary to keep the membrane moist; but either inflammation or irritation does immediately so add to its quantity, and so alter its colour, that the discharge from these parts has been taken for purulent, as if it was produced by ulceration.

As a Surgeon, whose memory is endeared to us, and whose fame will not soon yield to the lapse of time, says, "These two fluids, pus and mucus, which have been so frequently confounded together, do really differ so widely from each other in their nature, constitution, sources, purposes, and effects, that to distinguish them properly, and to point out the true character of each, seems to be a matter of much importance.

"Mucus, considered in a general sense, is the effect of a natural secretion made by glands, membranes, or other bodies appointed for that purpose; and is so far from being originally the consequence of dis-

ease, that, in a due quantity, it is absolutely necessary for several very important purposes in the animal economy; which purposes, when this fluid is deficient, must be ill executed, and some kind of disease or defect follow. Whoever will reflect upon the uses of it in the intestines, joints, sheaths, or capsulæ of the tendons, in the sinuses of the skull, serving the purposes of speech, in the cavity of the nose, where the olfactory nerves do their duty, in the Prostrate Gland, Larynx, Trachea, Urethra, and Vagina, will be easily convinced of the truth of this assertion, both with regard to its natural uses in a healthy state and proper quantity, and the share it frequently has in the production of diseases, when it is either vitiated or redundant.

“ Pus, or matter, is certainly no natural secretion; suppuration, though it is an act of Nature when some parts of the body have been forcibly divided from each other, is, nevertheless, to be regarded as the effect of violence or destruction, at least of division; for, without entering minutely into the origin or nature of it, I believe I may venture to affirm, that the dissolution of some of the particles of broken capillary vessels, and a mixture of some part of the juices which should circulate through them, make a necessary part of its production; however constant its appearance may be in the progress towards healing a wound, or sore, yet it never is produced, even in the smallest quantity, without some degree of erosion, some breach in the natural struc-

ture of the parts; and when such breach is healed, the discharge necessarily ceases.

“On the contrary, mucus may, by irritation, relaxation, or defluxion, on its secreting or containing parts or organs, be increased to a quantity far beyond what is necessary or useful, and produce thereby a disease in parts where there is not the least degree of solution of continuity, as in the cases of tenesmus, stone in the bladder, Fluor Albus, and simple gleet from the Urethra; as also in that kind of defluxion on the nose and fauces, producing a catarrh, and in the intermediate effect of all sternutatories.

“Other differences between the nature and properties of the two fluids might be mentioned; but if these already cited are just, they will be sufficient to evince the impropriety of confounding them together, either with regard to theory or practice.

“Inflammation and irritation of the membranous linings of the Urethra and Vagina, will fully account for all the appearances in this disease, in which there is neither matter, nor ulcer, nor abscess: whoever will attend to the discharge made from a purulent ulcer, will find it widely different from that which issues from either of the above parts in the Gonorrhœa.

“In cases of strictures in the Urethra, the discharge occasioned by bougie, properly and judiciously used, is a discoloured mucus, and not matter, though it is generally so called: it is from the discharge of

this mucus, and the dilatation of the passage, that the relief is obtained, not from any destruction or division of the parts: the bougie which produces true matter, does much more harm than good, and makes a sore where there was none, and where there ought to be none. How often do catarrhus defluxions on the trachea and larynx wear, towards the close of them, a deep purulent colour, so as to deceive the unknowing into an opinion that it is matter upon the lungs? But no judge of these things ever had recourse to abscesses or ulcers, for a solution of such appearance. The argument drawn from the quantity of these discharges is as erroneous as that taken from its colour; as an inflammatory defluxion on the part does generally occasion the latter, so mere irritation will produce the former, which does also generally cease when the irritating cause is removed or appeased. How immediately is a most troublesome tenesmus cured by a clyster of starch and opium? What large fœtid discharges are made from behind the prepuce of many persons, not only free from all venereal tint, but without any ulceration of the parts, by a kind of exudation? To what length of time will they not continue, if neglected; and how immediately do they cease by the use of a spirituous or vitriolic wash? How often is the Fluor Albus, even in some of its worst circumstances, moderated, not to say cured, merely by washing away the acrid mucus, which, lodging in the rugæ of the Vagina, continually irritated the

parts to a fresh discharge, and perpetuated the disease? What quantity of slime is there in the urine of those who have a stone in the bladder? And how totally does it cease, upon that stone being discharged or taken away? Whereas, neither cleansing of parts, nor removal of irritating bodies, does or ever can procure an immediate cessation of a discharge of true matter, which being occasioned by a solution of continuity, an erosion or division of the parts whence it proceeds, must decrease gradually, and at last can only cease by such part becoming whole again.

“In short, the two fluids are so absolutely different and distinct, that the blending them together, in our ideas of disease proceeding from, or producing either of them, cannot be too industriously avoided*.”

The difference betwixt pus and mucus, in my opinion, can never be reconciled; and I am at a loss to conceive how, besides this Author†, others have fallen into so outrageous a conceit—a conceit that militates against all common sense, and common observation. It must have originated from the lowest order of reasoning, from the mere similarity of colour of the two fluids: and upon the same pretence, as well might milk of roses, as it is called by the Perfumers, be applied to the purposes of cow’s milk, because the eye may be deceived in the distinction.

* Pott, on *Fistula Lachrymalis*, vol. I.

† John Hunter.

Although so much has been said upon the subject, merely as an answer to what has been advanced on the other side of the question, yet the true distinction, and all that ought to be regarded in order to point out the difference betwixt pus and mucus, lies in a very small compass indeed, which is, that increased discharge of mucus can only be found to flow from the surface of mucous membranes and the glands thereof, without any solution of continuity, but barely as an increased secretion ; whereas pus will be found on parts all over the body, where there is a destruction or solution of parts. This is the exact state of the case.

SYMPTOMS OF GONORRHŒA DEFINED.

A virulent Gonorrhœa, therefore, I define to be an increased infectious discharge of mucus, from the membrane which lines the Urethra in man, and Vagina in woman, and from the glands thereof. This discharge is occasioned by the stimulus of the venereal virus communicated to these parts, generally at the act of coition. When this discharge is perceived from the Urethra, it commonly appears within three or four, to twelve days, and sometimes longer, after the infection was received.

First of all, there is an unusual aching heavy pain about these parts, attended by a rotation of the testicles ; afterwards a fluid distils, by little and

little, from the orifice of the Penis, and appears in very small spots on the linen; or if the Penis be pressed by the finger and thumb, this fluid will be forced out, and the Urethra will feel painful and sore about the glans. As this fluid comes away, it occasions a titillation in the part; which sensation is likewise felt in making water in an increased degree, until at length it arises to a sensible heat, and a sharp cutting pain. This heat and pain of making water is at first also felt near the orifice of the Penis; and the lips of the orifice appear turgid, red, and open, and are exquisitely tender to the touch. The glans Penis also becomes hard, swelled, and glassy, and puts on a redder hue.

As the irritation spreads on the membrane, these symptoms are followed by others. The discharge increases, the sensation of pain is more acute, erections are involuntarily excited, or rather excited from a stimulus in which the mind has no concern, and are more frequent and lasting than what are termed natural erections. They are also attended with much uneasiness oftentimes, and the distended Penis feels at that time as if it were squeezed on both sides by some strong power. This symptom is most intolerable in the night time, when the patient begins to be hot in bed: it often awakes him from his sleep, and he is obliged to rise from his bed to allay it.

About this time also a Chordée is felt, which begins by a spasmodic action on the Frænum, during

erection; the pain of which is extended all along the Urethra, as if the Frœnum was upon a constant stretch; and sometimes in consequence of this strong action the substance of it gives way, and a considerable hemorrhage follows, which, for a time mitigates the symptoms. The discharge, at this period of the disease, generally increases; and especially after straining to make water, and just at the close of making water, the pain becomes more sharp and pungent. The discharge recedes daily from the natural appearance of mucus, changes both as to colour and consistence, and puts on the exact appearance of a discharge from the nose, in a cold.

When the disease is arrived at its most aggravated stage, all those symptoms become yet more acute; the heat of urine is so extremely intense, that the patient dreads the evacuating of it, when at the same time he is constantly soliciting to evacuate it, by a teasing uneasy sensation about the neck of the bladder. There is also, at this time, a constant heat and fulness all along the Urethra, and of the glans. The patient cannot sit down easy in his chair; pain is felt about the anus, and the preternatural action of the acceleratores urinæ make them tender when they are pressed upon; and the glands in the Urethra are swelled, and are often to be easily felt. The urine comes away in a small stream during the inflammatory height of the disease, owing to the general inflamed state of the Urethra; and when the swellings of the glands intrudes upon

the Urethra, the stream of urine will be scattered, and sometimes forked, owing to the irregularity of the surface of the duct through which it passes, and to the irregular action of the muscles in the expulsion of it. When the disease is arrived to this height, the most favourable of all appearances is a profuse quantity of discharge: for if the discharge was at this time to cease, other and worse symptoms, which will be hereafter described more at large, such as stranguary, inflammation of the prostate, of the vesiculæ seminales, and of all the deep seated parts about the neck of the bladder, would follow; and that they sometimes do, it is known from experience; and it is also known, that a kindly return of the discharge, is the most certain sign of their favourable and early abatement.

I have stated, in as concise a manner as I could, the gradual effects of an inflammation brought on from a venereal stimulus upon a mucous membrane, from its commencement at the orifice of the Urethra to the remotest part of it; and it appears that the discharge from the Urethra during the first inflammatory stage of the disease is expressive of the various degrees of inflammation, as well as of the extension of the inflammation along the surface of the Urethra. And as it is the case in other inflammations on other mucous membranes, so it is in this instance, that sometimes the inflammation proceeds to such an increased degree, that the surface of the Urethra and the glands within it, are ren-

dered incapable of yielding any secretion, and that during this stage of the disease, all discharge is of course checked. This is also the case in a severe cold ; and the safety of the patient, and his speedy restoration to health, is prognosticated by a return of discharge from the nose, and by a plentiful spitting, when the trachea and lungs have been oppressed. This is, in my opinion, the true path for pursuing the theory of inflammation on mucous parts ; and by this mode of reasoning we readily are enabled to assign a cause why a discharge from the Urethra is all at once discontinued, when at the same time the inflammatory symptoms are higher than ever.

I shall often allude to the theory of a late Writer on the Venereal Disease, without, I trust, being in the smallest degree personal ; for at any rate I would not be suspected of personality. My former publications were not only designed to inquire into his theory, but also into the motives which induced him to adopt some parts of that theory. But now I beg to be considered as an individual travelling on through the several stages of this disease, with the most anxious and heart-felt desire of acquiring honest fame from what I explore—to be considered as a bigot to no theory whatever—not so positive as to have the bias of my mind fixed necessarily to any other theory but that which I sincerely think the true one—not so positive as to adopt any innovation, but such a one as reason will support me in—and as deviating, with delicacy, from former opinions,

when the research will justify me in it, because I doubt their truth.

A late Writer has said, that the specific distance of the disease, or specific extent of it, is not farther than an inch or an inch and a half within the orifice of the Penis: for he says, "as the Gonorrhœa is commonly an inflammation, it is accompanied with pain, and the formation of matter; in such a state, neither the sensations of the patient, nor the actions of the parts themselves, are confined to the real seat of the disease." And then he goes on in shewing what other symptoms follow those original specific symptoms, and states them thus: "In consequence of the neighbouring parts sympathising, a variety of symptoms are produced, many of which do not exceed what may arise from an irritable state; an uneasiness, partaking of soreness and pain, and a kind of weariness is every where felt about the pelvis: the scrotum, testicles, perinæum, anus, and hips, become disagreeably sensible to the patient; and the testicles often require being suspended; and so irritable are they, indeed, in such cases, that the least accident, or even exercise, which would have no such effect at another time, will make them swell. The glands of the groin are often affected sympathetically, and will even swell a little, but do not come to suppuration: when they inflame from absorption of matter, they in general suppurate. I have seen cases where the irritation has extended so far as to affect with real pain the thighs, the but-

tocks, and the abdominal muscles, so that the patient has been obliged to lie quiet, in an horizontal position ; the pain has at times been so considerable as to make him cry out, and the parts have been sore to the touch ; they have even swelled, but the swelling has not been of the inflammatory kind, for though there was a visible fulness, yet the parts were rather soft."

It would be needless to quote farther, in order to exemplify what I wish to remark in my answer to this, for I have quoted sufficient for that purpose already.

I agree with this Author, that many of the symptoms, which I have stated from his publication, are not specific symptoms of the disease, but are the consequences of high inflammation of the specific parts upon which the venereal virus is acting. Inflammation or pain carried to a certain point cannot fail of affecting other parts of the same body besides that which is the principal in suffering. This must be readily admitted, and this is not a new observation, nor is it a new belief. But I am at a loss to know what has induced this Author to suppose that the venereal virus, which produces Gonorrhœa, only specifically acts an inch or an inch and a half up the Urethra? What is there in that part of the same mucous membrane which is continued through the whole of the urethral canal, so particular as to stop the progress of the true specific venereal action? If the real action of the virus has proceeded an inch

and a half, why should it not proceed farther, when it is to continue acting upon the same anatomical membrane? If it can go an inch and a half, it surely proves that it has the power of going on.

But what are our feelings upon this question, and what are the feelings of our patients? If some of us do believe that the virus can extend an inch, or an inch and a half only, will others be persuaded that what they feel beyond that mark, is only the effect of what is in reality acting up to that mark? Does not the inflammation throughout the Urethra appear to be the same? Is not the progress of inflammation through the Urethra regularly continued? Is not the discharge excited as well beyond as up to this mark? Is there any anatomical change in the membranes which commences there? What can be the inducement for such a new doctrine? It is very true, that when the disease is mild, and when the inflammatory symptoms are but of little moment, the inflammation may be confined to the part nearest to the orifice of the Urethra; but this must be only said to be the milder and incipient condition of the disease—and thus it will be with a slight cold on another mucous membrane: but it is as true, that the severity of a cold consists in the more general inflammation of that membrane, and that the severity of a Gonorrhœa also consists in a more general inflammation of the membrane of the Urethra.

Had this Author considered (for that is the present object of consideration) how it happens that any discharge was produced by a local application of venereal virus, a few days before—I say, had this Author considered that no such symptom as a discharge could possibly have been produced in such quantity and of such a nature, if the same venereal virus which excited it there, had been lodged and left to stimulate any other part of the body but that which was mucous membrane, he then would have been convinced, that the stimulus which had excited inflammation in a mucous membrane, and had increased the secretion of it, might proceed the length of that membrane: but beyond the length of the mucous membrane, in all probability, some of the symptoms which I have enumerated in my quotation, might be, and are the effects of the specific inflammation on the mucous membrane; for I am certain they never accompany a Gonorrhœa, when the inflammation does not extend more than an inch, or an inch and a half up the Urethra, but only when the inflammation throughout the Urethra is very high and alarming.

CHORDE'E

The Urethra is undoubtedly liable to inflammations, independent of those brought on from venereal virus; but these inflammations are not attended

with such high symptoms as those which are produced from a virulent Gonorrhœa. One of the most troublesome of all is that which is called a Chordée, which takes its name from the Penis being curved downwards, and the glans drawn down as it were by a chord. This symptom is most generally felt when the inflammation runs high, and is much more troublesome to the patient in his first Gonorrhœa than in any contracted afterwards. This symptom is chiefly experienced in bed, and when the Penis is in a state of erection. It is a circumstance of no little mortification, that the Penis should have the strongest propensity to erection at the time when only it were wished that such a power would fail — for the rest is disturbed, the symptoms are aggravated, and the disease generally protracted to some length, whenever a Chordée becomes one of the symptoms of a Gonorrhœa.

The Chordée is certainly produced from an inflammation of the substance of the Urethra, from the inflammation having penetrated beyond the surface, and affected the corpus spongiosum Urethræ. The corpus spongiosum Urethræ being inflamed, becomes painful and tumid, the circulation is impeded in it, and the power of extension restricted. The corpus spongiosum Urethræ, in a state of health, confirms and corresponds in action with the corpora cavernosa Penis. When the corpora cavernosa Penis are in a state of flaccidity, so is the corpus spongiosum: and when the blood is thrown into the corpora

cavernosa, so as to increase their bulk, and cause thereby an erection of the Penis, the corpus spongiosum also receives its share of sanguineous aid, and completes the intention. This is often the effect of the will, often the consequence of venereal reflection, sometimes an act from the heat of the bed, from an inflammation of the part, and from a stimulus on other parts which correspond with the Penis. It is a venereal stimulus from a mental idea which produces the erection in a state of health — and it is an inflammatory stimulus of the parts which produces the erection in a state of disease.

In this instance of Chordée, the corpus spongiosum Urethræ is so highly inflamed, that it cannot bear distension, and it is the act of distension which produces the pain that is felt; for the corpora cavernosa, not at the same time inflamed, are capable of their usual distension; but when the blood is flowing into the reticular substance of the corpus spongiosum, the vessels of it are found to be inflamed, tumid, and too much loaded; the effort, therefore, cannot be even attempted, without producing great pain, and without sometimes lacerating the reticular substance, from which a considerable hemorrhage ensues, and which is, in general, found to be a relief. It is when the corpora cavernosa are swelled with blood, that the compression of them upon the corpus spongiosum produces a feeling as if the Penis was strongly squeezed between the finger and thumb. This is my Theory of Chordée.

The *ardor urinæ*, or heat of urine, is a sensation produced, not from the urine being more hot at that time than at any other, but from the change of condition in the canal through which the urine passes. This sensation is brought on from a change in the condition of the mucus, owing to the action of the venereal stimulus on the membrane. It is not only felt all along the Urethra, but more especially at the sphincter vesicæ. It is there so exquisitely sensible, that a constant dread of making water is always uppermost; and then it is that drops of blood are seen to follow the urine, and that they are also seen mixed with the discharge upon the linen. Besides the pain and bleeding attendant upon this state, the sphincter vesicæ is so swollen, that it cannot, when the effort is made for closing it after urining, fall completely into those regular folds which it does in a state of health; and from this cause some drops of water are found to issue for some time after the intended action of urining ceases, and some drops also precede that intended action, from the difficulty being as great in its dilating as it is in its contracting. It is owing to the sphincter not being completely closed, that a little of the urine constantly finds its way out of the bladder, and keeps up a successive solicitation to be discharged.

I have before remarked, that the worst stage of a Gonorrhœa consists in the inflammation of the membrane of the Urethra, of the glands within it, and of the neck of the bladder being so very power-

ful as to produce a cessation of the discharge. When the secretions are, from this cause, at an end, there is in general, in addition to great heat along the membrane—to the tumid appearance of the visible part of the Penis—to the constant propensity of making water—a dull continual uneasy sensation, seated near to, and affecting the anus, which I suspect to be inflammation of the prostrate gland. The patient at this time cannot bear to sit down, nor scarcely turn in his bed; and when he moves, he walks with his legs straddled. Throughout the whole symptoms of the Venereal Disease, I do not know a more critical situation than the patient is in, thus circumstanced; for whatever lasting injury is done to those parts, either to the neck of the bladder, or to the prostrate gland, it is often entailing continual disease, which baffles our power totally to remove for ever after.

There is another symptom attendant upon a suppression of the discharge, and which is also a symptom during the inflammatory condition of the parts, when the discharge continues, namely a swelling of the inguinal glands. The swelling of the inguinal glands has been said by some, to be symptomatic, by others sympathetic (a term now become very familiar to the ear in Surgery, more so than it ever will or can to the understanding), and by others, to be the effect of absorption of the venereal virus. It is observed, when the glands become tender and swell, during a continuance of the discharge, that they generally

abate of both, as the inflammatory symptoms in the Urethra decline, and as the discharge assumes a more kindly condition — I say this is the case, in general: and also, when these glands are found to become tender and swell, whilst there is a total suppression of the first discharge from the Urethra, they have, upon a return of the second discharge, and as it becomes more kindly in condition, been found, in general, to abate of both these symptoms. But in both these instances, exceptions do happen to the contrary. The glands will sometimes come to abscess, even whilst the discharge from the Urethra continues; and they will oftener come to abscess when the discharge does not return any more. Those who with me suspect that Buboës thus forming, and thus coming to abscess, are venereal, will doubtless treat them after a serious manner, and consider them as the seat of infection, advanced at least a stage beyond the original point where the first local infection took its date, and treat them accordingly. Whilst those who consider them as a symptom which is the effect of corresponding affection or irritation, or sympathy, will follow up their theoretic opinions in practice, and treat them as common simple glandular abscesses, as innocent tumors, neither requiring mercury, because they are not venereal, nor any other species of alterative, because they were not produced from Scrophula, Scurvy, or any other obstinate, indurating, chronic cause, but merely from the effect of sympathy in parts.

ON PHYMOSIS AND PARAPHYMOSIS.

There are two other symptoms arising from Gonorrhœa, which a late Author* has said nothing about. A Phymosis and Paraphymosis, which are excited from a Chancre on the Prepuce, and where a Chancre was the first cause, he has treated with the usual perspicuity that he has treated every other subject, but not that Phymosis and Paraphymosis which arise from Gonorrhœa. Where a Phymosis arises from Gonorrhœa, it begins without any apparent provoking first cause but Gonorrhœa; no Chancre is ever to be seen, but in all other respects the symptoms of it are equally aggravated as when a Chancre was the first cause.

A Phymosis is in part a soft œdematous intumescence of the duplicature of the Prepuce, which is supplied by exudation of lymph on the part, and lodged there in too redundant a proportion. In consequence of this intumescence, the Prepuce is so thickened that it cannot be slipped back, hangs in a considerable portion before the glans Penis, and prevents it from being denudated or seen.

A Paraphymosis is in consequence of the like intumescence, and from the same cause. It occurs where the Prepuce is naturally very short, and where it has been slipped back whilst the Prepuce was com-

* John Hunter.

mening to swell, and before the swelling had increased to any great degree. The swelling still continuing to increase the difficulty, a cure of the Prepuce over the glans is found afterwards to be impracticable. If the Prepuce be suffered to remain in this state, it will produce so much stricture on the substance of the Penis, as to cause it to slough entirely away.

Thus a Phymosis and Paraphymosis are the same complaint, in different positions. I said that the tumid feeling of the Phymosis was partly œdematous only, for there are parts of it which feel much harder than others, which I conceive to be the swelling of lymphatic glands; and it oftener arises from Phymosis or Paraphymosis, than from any other cause, that we trace hard and knotty chords along the dorsum Penis. These will remain for some time after the Phymosis has subsided, and will sometimes terminate in small abscesses. These hard chords are lymphatic glands, either irritated from corresponding inflammation, or from the infectious lymph passing through them. These hard chords are not confined to Phymosis or Paraphymosis only, but are also, in consequence of exudation of lymph around the glans Penis, and Chancre.

A young gentleman, about two years ago, applied to me, for Gonorrhœa: the symptoms of it were of the moderate degree: the prepuce of his Penis was long. It was in the Summer time, and he went, about a fortnight after his first application to me,

in the water, when he was caught in a shower of rain, and the next day he found himself feverish, and chilly: at that period his discharge disappeared, and his prepuce began to swell. It continued to swell as much or more than in any Phymosis I had ever seen. I could feel, amidst the œdematous swelling, on two or three points, hard tumours. But although he was confined to his house for nearly a fortnight, and although he had a second return of the Phymosis from indiscretion, yet there was no appearance of an ulcer from the beginning to the end of his cure. In this case there were three large hard chords along the dorsum Penis, two of them had at first made up one tumour; but as the tumour subsided, the two knotty glandular substances could be with ease distinguished.

The cause of this Phymosis was as wide from that brought on through Chancre, as it is possible for two causes to be. This was positively an inflammation, extended to this part, in common with that inflammation in the Urethra, which gave cause, for a time, to the total suppression of discharge from the membrane within the Urethra.

SWELLING OF THE TESTICLE.

During the progress of a Gonorrhœa, a swelling of the testicle sometimes comes on as a symptom, which in itself is of a very troublesome nature. It is scarcely ever attended with the loss of the testicle,

nor does it often leave behind it any hardness which does not subside, or which prevents a restoration of those functions it is the property of the testicle to perform. That Gonorrhœa is the cause of the swelling of the testicle, is a self-evident fact ; but how the effect is brought about, is a question still agitated, and perhaps, as every reason which has been hitherto assigned fails in the explanation of satisfying doubt, it might be suspected that the true reason is not as yet satisfactorily laid down, in accounting for this important event.

I shall first of all endeavour to describe at what period of time, and in what condition of a Gonorrhœa, a swelling of the testicle generally comes on. It may be remarked, that I constantly cover every assertion which I advance, by saying that it generally happens, or that it is a general symptom. I certainly do so, and I justify myself in it, because from things as they generally occur, we are to form our conclusions, if we mean to come at the truth. Nothing leads men more into error, and especially and constantly into medical error, than drawing conclusions for a general scale of reasoning out of rare cases, and rare circumstances accompanying rare cases. It now becomes the fashion, and I will venture to say, that the only road to error is to lose sight of common occurrences, and to draw conclusions from uncommon cases.

A swelling of the testicle generally comes on at the latter stage of a Gonorrhœa, after the inflam-

mation has marked its progress through the surface of the whole of the Urethra: it generally also comes on at a time when the patient thinks himself getting well apace, when inflammation has subsided, where it had been most felt, and when the discharge has lessened also. This phænomenon in the animal œconomy has excited various opinions for developing the cause which produces it; and yet I do not know if that the true cause of the attack of the testicle, in this instance, were exactly defined, whether it could be prevented oftener from coming on, or whether it could be cured by a more simple and compendious manner, than it now is, when properly treated.

At the period when pain is first felt in the testicle, and when the epididymis begins to swell, the discharge from the Urethra generally ceases. If it cease wholly, the inflammation in the testicle goes on; but if at that period when the testicle feels tender to the touch, the discharge has but partially ceased, there then is a probability, by the omission of injections, if they were in use, by suspending the testicle, by horizontal position, by opening the pores, and by moderating the diet, that the discharge may return, and the tenderness of the testicle abate. But when the contrary is the case, the progress of inflammation is as follows: A pain is felt in the epididymis, which is exquisitely sensible to the touch; it is swelled and hard; the body of the testicle does not for a few days appear in some to be

affected, in others it does immediately after the epididymis : but the most exquisite sensations of pain are felt when the testicle itself swells, and this pain I am certain is owing not so much to the swelling of the substance of the testicle, as it is to the distention of the coat of the testicle. The scrotum sometimes will also partake of the inflammation, and appear red and thickened.

After the testicle has experienced the effects of swelling and inflammation, the spermatic chord swells, and is also exquisitely sensible of pain, the pain is traced all along the direction of it, and the loins and kidneys become in their turn also sensibly affected. The pain in the back often excites the necessity of shifting positions in vain for ease, a general symptomatic fever prevails, the pulse is quickened, and sometimes the patient will be delirious ; in short, the symptoms in some irritable habits do become so very alarming, that if we were not able to assign a cause for them, the fever would assume every appearance, and be judged to be of a dangerous nature. But as it is the case, in all fevers excited from an irritation on a specific part of the body, the pulse, although extremely quick (for I have felt it beat one hundred and twenty strokes in a minute), will be irregularly so.

The most disagreeable symptom of all in a swelled testicle is a sickness of the stomach, with vomiting. This action of vomiting, when the fever runs high, becomes additionally aggravating by the pain in the

loins, and by the increased pain of the testicle. It very rarely happens that the testicle comes to abscess, but it sometimes does, scarcely ever, I believe, but from neglect or wrong treatment. If any thing independent of constitutional disposition can bring on a swelled testicle, I think riding on horseback, excessive walking, stimulating purges, sharp injections thrown up in the height of inflammation, and obstruction of the natural perspiration will effect it. But nocturnal emissions, or any act which produces seminal discharge, during the inflammatory stage of a Gonorrhœa, will, above all other contributory causes, tend to bring forward a swelling of the testicle.

Here we see a certain train of inflammatory and consequent symptoms following each other, from the first irritation, which is in the testicle, to a more general irritation of the whole of the body, corresponding with, and existing so long as the pain and inflammation of the swelled testicle continue, and no longer—and this is the true idea that I have of a symptomatic or corresponding irritation or affection. But we do not ever find that this inflammation exists, and is continued in the Urethra at the same time that the testicle swells; but, on the contrary, the inflammation, pain, and discharge in the Urethra all cease, and the Urethra appears to be free of every degree of irritation at the very time when the swelled testicle is at its highest and most exasperated state of pain. And moreover than this, when the inflam-

mation of the testicle abates, and the swelling subsides, when the spermatic chord is no longer affected, when the pain of the loins is no longer felt, when the pulse is calm and the stomach at ease, although the swelling of the testicle be not gone down, it is then we find that the discharge from the Urethra returns, attended sometimes with as many symptoms of inflammation, as were attendant upon the discharge before the testicle had commenced to swell. If this description be the true and general description of the condition of those parts, whenever the testicle does swell, I cannot think such effects are to be attributed to sympathetic affection, or to what is termed sympathy; for the fact seems to be so opposite to such a doctrine, that it may rather be termed antipathy than sympathy. When I consider the state of the Urethra, with the state of the testicle, when the testicle first inflames; or, when I consider the state of the testicle, when the Urethra inflames a second time, after the discharge again returns, so little does the condition of the one correspond with the condition of the other, that they never appear to suffer together, but to suffer alternately. The one part seems to be eased by the pain of the other: when the Urethra is in pain, the testicle is at ease; when the testicle is in pain, the Urethra is at ease. They are the Castor and Pollux of each other. I say this is not the general effect of corresponding irritation, nor of sympathy. This is not similar to the effects of swelled testicle on the parts which I

have lately enumerated, nor similar to the effects on the spermatic chord, on the loins, on the stomach, or on the pulse. These were not the most affected when the testicle was the least, but were most affected when the testicle was so, and particularly and only at that very point of time.

This is my idea of corresponding irritation, and this must be the only true idea of sympathetic affection, whenever those who chuse to adopt that term in reasoning upon this subject, or any other similar to it, can adopt it with truth. I therefore give it as my opinion, for the above reasons, that when the testicle swells from a Gonorrhœa, it is not owing to sympathy with the Urethra. If it were owing to sympathy in parts, whenever it came on, it would be always when the Urethra was most inflamed, when the scalding of the urine was most acute, when the Chordée and erections were most troublesome; and the swelling of the testicle would abate, as these symptoms abated; — but unfortunately for the Author's opinion, in this instance, who first avowed this doctrine, the testicle never scarcely is known to swell whilst the Urethra is found in this condition.

The English Authors highest in fame, for purity of diction, as Locke, South, and Johnson, are known to define sympathy — to be an affection of one person for the condition of another. Now if in addition to this power of sympathy, we apply the term to the affection of one part for the condition of another part in the same person, and vainly annex to

sympathy, in this instance, not a mental idea, but a real corporeal action, that part which sympathises, must necessarily be suffering for the condition of the other, at the time it suffers. For it cannot be said to be sympathy, when this reciprocity ceases, provided the mutual suffering even began in sympathy. I can only say thus much with truth, that the pain of the testicle most effectually goes off, that the swelling of the testicle most rapidly declines, when the second return of the discharge from the Urethra is the most profuse. It is not material to the progress of the reduction of the testicle, whether there be a scalding of the urine, or any other inflammatory symptom attendant upon the second return of the discharge; but it is solely dependant upon the plenteous return of discharge, either with or without (indifferently which) inflammatory symptoms, that the success of the reduction of the testicle depends; for when the discharge ceases before the swelled testicle has subsided, the remaining swelling is generally a long time before it goes down, and sometimes it never does. We ought to attend to this circumstance, and to do nothing to stop a discharge, whilst a swelled testicle is subsiding. And it is in such instances that ingenious men, knowing this to be the fact, have devised stratagems to bring on a discharge.

A certain modern Writer on this disease has also asserted, that he has known the testicle to swell, when the inflammation in the Urethra has never

extended above an inch or an inch and a half up the Urethra. I confess I have known no such instance; but if I had, I should draw no conclusions from it which would affect more constant and more general observations to the contrary. Testicles, he says, are known to swell from other causes, besides that from Gonorrhœa. Nothing, in my opinion, from this circumstance, can lead to inform us what it is which produces the swelling in this case. When the testicle swells from the use of bougie, and the circumstance is very singular, and worthy our notice, it is then generally one only that is affected. In case of a scirrhus, it is also in general one only that is affected. And in the case of swelling from Gonorrhœa, we find that although sometimes the inflammation does shift from the one to the other, yet if the attack be serious, and if the inflammation proceed the whole of its extent, there is seldom an instance when both testicles are highly inflamed at the same time. We find also when the testicle swells from any cause, unattended with a discharge from the Urethra either before or after, that the hardness continues sometimes for ever after all inflammatory symptoms are gone.

Such being the natural occurrences which offer to our observations upon the swelling of the testicle, it appears that Nature has been very provident with respect to this double security for propagation. And although it seems almost unreasonable to suppose that the orifice of one vas deferens can be affected

from a venereal stimulus, and by that means that the testicle to which it leads can be, in consequence, affected, whilst in the very midst of inflammatory stimulus, the other vas deferens and the other testicle shall escape the effects of venereal stimulus—I say, although this appears strange, yet we know it to be a fact; that is, we know that one vas deferens will feel tender and affected, and that one testicle will swell, without the other being in the least affected at the same time. Such is the fact, let it be ever so difficult to be reconciled to our understanding: and I should suspect that although the orifices of the vasa deferentia may be a bar to the actual progress of the virus in general, and although from an extraordinary instance, that bar, in the provision of nature, might prove to be inefficient, and a swelling of the testicle, from that circumstance, does ensue, yet if sympathy had any concern in the condition, both must swell when one only does.

Besides, when the use of bougies brings on a swelling, seldom more than one testicle swells. This seems as if it were an original design in the disposition of the parts, from the very nature of them, that both should not be seriously affected from the same cause, at the same time; or how otherwise can we account for the attack upon one, and the escape of the other? The swelling of the testicle cannot, in my opinion, be affected from a bare stimulus of the venereal virus acting on the

orifices, and no farther, of the vasa deferentia; for if that were the case, a long-protracted Gonorrhœa, and such as had excited the full effects of inflammation on the neck of the bladder, on the prostate gland, and on all the parts deep-seated in the Urethra, would surely be a sufficient cause for provoking both testicles to swell, whenever such inflammation occurred: but this we know is not that which usually happens, for I believe I may venture to assert, that a swelled testicle does not occur to one in twenty of those thus conditioned.

It evidently appears, from these observations, that there is a difficulty in the effects of a swelling of the testicle being brought about. I am, therefore, of opinion, that the orifices of the vasa deferentia, which open into the Urethra, are shut against the effects of all stimuli; and that it is from accident alone venereal stimulus can possibly be admitted. But if it once gain admittance, I am of opinion that the venereal stimulus can act as well along the vas deferens, as it does along the Urethra; and that it is the positive presence of the venereal stimulus which produces an inflammation on the internal canal of the vas deferens, and which proceeds to the epididymis, and affects that and the testicle. If virus can pass through lymphaticks, by what is termed absorption, there is no difficulty to presume that it may pass along the vas deferens, by capillary attraction. And I am also of opinion, that part of the discharge which follows a swelled testicle, and

to which a swelled testicle from a venereal cause owes its restoration to a sound state, flows through the vas deferens, and that it is poured from thence into the Urethra. It does not follow, from this mode of reasoning, that this discharge should carry with it any different mark from that which flows from the Urethra; for an increased stimulus upon the internal membrane of smaller tubes will produce an increased discharge from them, alike as it does from the Urethra. The more the discharge which follows the swelled testicle, the sooner will the testicle go down; and this is a reason why a swelling of the testicle, from a venereal cause, in general subsides sooner and safer than a swelling brought on from any other cause, and where no discharge follows. I am also of opinion, that when the venereal stimulus gains admittance within the orifice of the vas deferens, the progress of the inflammatory symptoms are as slow there, as they are through the Urethra after a gonorrhœal infection has been first received.

From the above statement of the cause of a swelled testicle, I think we have been able rationally to account for the various circumstances attendant on the swelling of the testicle brought on by Gonorrhœa—how it happens that sometimes, notwithstanding long and great inflammation in the Urethra, the testicle is not affected at all—how it is from accident when it is affected—how one may be affected without the other—how that affection may come on

after inflammation in the Urethra has subsided—and how its own cure consists in the return of the discharge.

GONORRHŒA OF THE EYE.

THERE are two distinct conditions of venereal affections of the eye, the one arising out of constitutional infection, and the other from a local infection. It is the Gonorrhœa of the Eye, or the local venereal infection, which I am now about to treat upon. The Gonorrhœa of the eye is one of the most acute, inflammatory, painful, and destructive attacks which can be endured: in comparing it with the whole class of acute diseases that locally affect human parts, I am not able to recollect another equal to it, in rapidity, torture, or destruction. Two cases of this unhappy attack have come before me: in the first the eye was lost, and in the second, from timely treatment, and from being aware of the nature of the complaint, the eye was with difficulty saved. The virulence of the attack of the latter was nothing like so formidable as the former.

This attack of the eye has been attributed to a metastasis, from a repelled recent Gonorrhœa; and it has been described as if the Gonorrhœa from the Penis ceased when the Gonorrhœa from the eye came on. That when the pain in the eye becomes intolerant, the pain in making water, and other inflammatory symptoms of the Urethra will

be sensibly diminished, it is consonant to general observation in other similar instances, which present themselves in the animal œconomy. In these two cases the discharge from the Penis was not discontinued, but the inflammatory symptoms were not high. I do not believe this doctrine of metastasis. I believe the venereal virus was conveyed by the fingers; for both my patients were previously subject to weak eyes; and the former, he who lost his eye, was in the habit of washing it with his urine, to cure it of the weakness.

I am now to point out to you, how it happens, that out of so many which we constantly see infected with Gonorrhœa, who are totally indifferent and thoughtless about any consequences, and who therefore do not make use of any precautions, none infect themselves through wounds and sores, and chops in their lips; and that this complaint of the eye is so seldom experienced. The fact is certain, that the instances of Gonorrhœa of the Eye are very rare indeed, and that the instances of wounds, or sores, or chops on any parts of the body, being infected, notwithstanding they are exposed to such infection, if it could act, are rarer still. We all know that not only we have the habit, but that it is a natural addiction in us to be constantly fingering the parts which are diseased. And yet I cannot help remarking, for the observation intrudes itself upon me, that we are never infected on fresh parts in this manner. Notwithstanding, if the infectious

discharge could act, we are more exposed to infect ourselves, than others are to infect us. I must here explain myself, in order that my true meaning might be only impressed. I am assured, in both the cases which I saw, that the fluid from the Penis of the first or second patient did not produce the inflammatory effects on their eyes: for I know that the gonorrhœal fluid of the same subject is not capable of producing that effect. And therefore this theory accords with the observations I have made before, on the nature of both gonorrhœal and chancreous fluids, as they affect the same subject. For, if a Gonorrhœa from the Penis of the same subject can produce a Gonorrhœa of the Eye, the Gonorrhœa of the Eye can produce a Gonorrhœa of the other eye, and the infection can go on in an endless round of diseased parts. Wherever the discharge gained a lodgment, if it were virulent, there it would commence an action conformable to the part, and the whole of the body would be one scene of Chancre and Gonorrhœa. But this is not the character natural to this or to other poisons. Where a subject has a Chancre or a Gonorrhœa, as a first local symptom, the virus which produced it was received from another, that virus necessarily proceeds in its action if it be not extinguished; and it is that virus, imparted from another, which is absorbed into the habit, and there again it produces all those symptoms which occur in the history of the disease, and to which the subject who imparted it was in like-

manner liable. Of this theory, however new, as applicable to the present subject, I am more convinced of the truth, than I am of any idea I ever found myself adequate to form. The poison of a viper does not affect the viper which secretes it, but it will affect another viper, or any other animal into which it is suffused. It is an excreted fluid, lodged temporally in an excretory bag, in order to be discharged whenever the viper is provoked to it.

I shall close this subject with an account of the case of a gentleman who lost his eye from a virulent Gonorrhœa. A gentleman was recommended to me for a virulent Gonorrhœa: the first time I saw him he complained also of one of his eyes being more inflamed and more painful than it ever had been, and this was the next day after the Gonorrhœa had discovered itself. I bled him for this inflammation, and gave him the necessary remedies for the Gonorrhœa Urethræ.—I saw him the next day, when I found his eye more inflamed, and the inflammation of the Urethra lessened. I bled him again, fomented and poulticed his eye. The next day the pain from the eye was increased, he complained of a violent throbbing, the discharge from it was in increased quantity, and looked purulent, the tunica conjunctiva was in a higher state of inflammation than I ever had seen one before: I applied leeches to his temples, blisters behind his ears, injected milk and water, renewed poultices, &c. The inflammatory symptoms in the Urethra at this time

were of no importance, and the discharge from it was kindly. The morning following I found the pain of the eye had not in the least abated, the conjunctiva assumed one continued fleshy appearance, which protruded so much as to leave the eye-lid more than half open. The same remedies were repeated, with the addition of mercury and opium. The next day, the pain and discharge were yet more increased. The inflammation not yielding to any of these revulsive means, I repeated all the remedies which I had applied the day before. The next day, the symptoms continuing without the least abatement, I solicited a consultation; but the Surgeon did not meet me till the day after, when every appearance was yet more aggravated. We proposed touching the carnosus fungus of the conjunctiva with fine powder of mercur. calcinat. mixed with soft pomatum, by a camel brush. This increased the pain so much, that we directly desisted, and returned to our former method a day or two longer, when the pain gradually abated, and the carnosus appearance began to decline. We then gave him bark, and continued mercury, which succeeded very well in taking off the inflammation, but the issue was, that the cornea was thickened, the crystalline became opaque, and the patient lost all vision. The Gonorrhœa from the Urethra by this time got also well. At the time when this case happened, I found no assistance from any Author how to treat it, or how it had been described by others, excepting

from Astruc, and he advises the tunica conjunctiva to be freed from this fleshy fungus by the knife.— If I were to treat this case again, I should certainly make a puncture through the conjunctiva, in order to give an opening for the discharge of extravasated lymph. But how this fungus is to be dissected away, during the torture and pain which the patient undergoes, I know not. It seems to me to be impracticable.

It appears in this case, that the symptoms of the Gonorrhœa of the Eye were as forward or early as the symptoms of the Gonorrhœa of the Urethra. It must be also remarked, that though both eyes of this patient had been previously weak, yet only one of them was thus affected. The patient had been in the constant habit of washing his eyes with his own urine, for this weakness, and he had washed his eyes half an hour after he had had connection with the woman who gave him the Gonorrhœa. My opinion is, that it was the positive fluid which he received from the woman, that infected his eye, and that the other escaped, because it was the last which he washed, and because none of the original infection came into contact with it*.

* Whether the Ophthalmic Inflammation which began in Egypt, and which was continued for some time after the return of the soldiers to this country, has been considered to be positively Venereal or not, I cannot say, and as I do not wish to make a positive assertion upon this question, I shall be content to dismiss it upon opinion only. For as I was not there myself,

GONORRHŒA IN WOMAN.

GONORRHŒA in woman is not frequently attended with symptoms so complicated or so violent as those in man. The cause is assignable only to the different construction of the parts which are exposed to an attack of this disease. But although the symptoms are neither so complicated, nor so lasting, nor do they bring on other after consequences, which are in themselves more alarming in their nature than the original cause which produced them, yet sometimes enough of inflammation and enough of pain are found to accompany this disease when it first appears in the Vagina of woman.

It need not be observed, that as it is the most usual manner for man to receive the infection on his

and as those who were there have varied in their opinions, it would be presumption in me to make a positive declaration upon the question. But after having enquired into the following facts, and after having satisfied my mind upon them; namely, that the disease did spread not only among the soldiers in Egypt, but that it continued to infect soldiers in England that had not been there; that wheresoever the soldiers infected in Egypt were quartered, soldiers not from Egypt became infected; that some were only infected in one eye, and others in both; that officers who served in Egypt generally escaped the infection. These reasons confirm to me an opinion that the disease was *VENEREAL*. That it was communicated from one to the other by lying in the same hammocks, by washing in the same basins, and by wiping themselves with the same towels.

Penis from the Vagina of a woman, so does the Vagina of a woman receive it from the Penis of a man. In some instances, a simple Gonorrhœa may be confined to the Vagina, hardly any other part will be susceptible of the stimulus, and a woman will have a Gonorrhœa without knowing that she has one. This simple mild appearance of the disease is generally experienced by women in the common habit of venereal intercourse. But women of a contrary way of life are more sensibly irritated by venereal virus, although with them, according to their constitutions, there will be a sensible difference in the affections produced by inflammation. The most general symptoms of a Gonorrhœa are an inflammation along the Vagina, with swellings of the labia, clitoris, nymphæ, carunculæ myrtiformes, excoriations on the perinœum and on the meatus urinarius. These are attended with pain, itching, scalding of urine, immoderate discharge, fever, uneasiness in sitting and in walking: the labia are enlarged, become rigid and protrude. Such are the symptoms which appear in the first instance, and which seem specifically to embrace all the effects of venereal stimulus at one and the same time, and are, excepting the general fever which they produce, independent of any corresponding irritation arising out of one of them simply for the condition of the other. And here also it may be remarked, that let those symptoms grow to any height, they do not produce chancres on these parts, nor on any other conti-

guous parts to which the gonorrhœal fluid might have easy access, although the discharge will often excoriate.

Sometimes the inflammatory symptoms are not confined to these parts alone, but are found to extend to all the region of the lower part of the abdomen, producing pain, and so strong a sense of tenderness, that the patient cannot bear the least weight of clothing, also violent intestine commotion, flatulencies and vomitings, attended with a symptomatic fever. It has been presumed that these effects are owing to the uterus being affected, as well as the ovaria.—It may be possible that both may be affected from the disease: But where I have seen those symptoms, I was inclined to attribute them to an inflammation on the bladder, for the chief seat of pain was directly described to be above the pubis.

Where the inflammation has gone on for a considerable time, the glands withinside of the labia and other parts will remain enlarged and hard, and sometimes never return to their healthful size, and sometimes they will come to abscess. It also happens in a woman as it does in a man, that either from irritation or absorption, the glands of the groin will swell and proceed to abscess.

These are the chief effects of a Gonorrhœa in woman; and as the inflammation on these parts is to be accounted for upon the same ground of reasoning which I have already explained, and as the same method of treatment is requisite for both, a

further enquiry into the subject of what has been already described will perhaps be unnecessary.

The most important topic for medical enquiry into these affairs of women, does not consist in a clear, avowed knowledge of gonorrhœal symptoms ; for these being once confessed, the case is plain, and the path for removing them will be as plain also. It is not in this instance that address and penetration are so requisite, as it is—where the case is doubtful—where the possibility of giving infection is denied—where a medical man is called upon to decide upon the condition of a woman, whether from that condition she be in a state for conveying infection or not—where he may have, from his discernment, her arts to baffle, or her innocence to protect—or where her ruin or her good fortune is at stake upon his judgment. This is a confidence, a part of medical jurisprudence which carries with it importance, and a man of sense will feel it in a high degree ; he will aim to be furnished with every distinction which may possibly lead him to a right determination, and which may teach him to discern between the tricks of art and the distress of innocence. The only proof I know of a woman being infected, when all inflammatory symptoms have disappeared, is, that she retains the power of infecting ; for a discharge from a Fluor Albus, and a gonorrhœal discharge, when free from all inflammation, will not afford a distinction for me to conclude them to be different. Perhaps those who know the one to be

pus, and the other to be mucus, may be able to discern a difference.

It has been said that a woman will go on with a Gonorrhœa for a long time—that the power of infection will be demonstrable to a stranger, a long while after it ceases to affect her more constant friend. This I believe to be true ; and in this instance the woman cannot be blamed, although she can and does infect a stranger. A woman might also have been free from all infection, and have known a man who was infected ; she might have received an infection from this man ; immediately afterwards she might have known another man who was not infected ; she might have imparted the infection which she received from the first to the second without being conscious of having done so : for the gonorrhœal fluid which had been lodged with her had not time to produce any stimulus ; and it might be shifted altogether to him who had the second connection. In this instance, the woman cannot possibly be blamed, as not knowing herself infected. But on the other hand, the woman may know that she can infect, because, although her inflammatory symptoms are just gone away, she has still a considerable quantity of discharge. It is in this situation that she will often keep back the truth, avow her innocence, and set detection at defiance.

CHAPTER VI.

CURE OF GONORRHŒA.

THE cure of Gonorrhœa must naturally include a consideration of the nature or quality of the part attacked, and a consideration of the nature or quality of that which attacks it, and produces an inflammation upon it. Any common stimulus, which is not of a virulent nature, applied to a mucous membrane, will produce some degree of irritation ; but such irritation will cease by the stimulus being carried off in the secretion. The ardor urinæ excited by the action of cantharides can only last for a short space of time ; because, from the moment the sensation is felt, the secretions are diluting the stimulus, and carrying it out of the constitution, and when the action is totally spent, the sensation from it is discontinued also.

Mucous membranes are placed as coverings to parts which are inlets and outlets of the constitution, and in their construction are so framed as to express a sensation whenever any extraneous injury is offered to them, or whenever any extraneous body is lodged upon them. Effluvia, which is inimical to the welfare of the body, passes over the nasal membrane, not without notice of disgust, by the smell which is

impressed upon it ; and irritating properties lodged upon it, are gotten rid of, or an endeavour is made to get rid of them, by the additional secretion which is excited. Mucous membranes seem to be, by destination in the animal œconomy, sentinels to the rest of the body, and endowed with power to alarm the constitution whenever any thing obnoxious is liable to assail it.

It has been said that a Gonorrhœa will cure itself, and it has been also said, that a Gonorrhœa cannot be the cause of Lues Venerea in the constitution. Both these assertions, if they were founded in truth, would be very fortunate ones indeed, and if they were founded in truth, there would not be so strong a reflection upon the morality or importance of a profession in all the round of human occupations. That a Gonorrhœa may cease to be a Gonorrhœa, if let alone to its own action, may be very true, but it may also be as true, that it might not cease to be Gonorrhœa, until it had reduced the organization of the Penis to a condition which could not afterwards be restored to a sound state. The Urethra being endowed with the faculty common to every other mucous membrane, of getting rid of that which irritates, and of being a barrier against whatever might, if it got into the constitution, annoy it, does certainly contend very strongly with that which irritates, and as often gets rid of the irritating cause ; but in the instance of the stimulus excited upon it by venereal virus, it appears from

evident facts before us, that the difficulty is greater than from any other cause whatever ; for what other stimulus, applied to a mucous membrane, is there, which will continue so long, and which will baffle all medical means so effectually, as the stimulus which is excited by venereal virus ? What other stimulating property is there within our knowledge, that by once applying it to the Urethra, will afterwards support a stimulus in the Urethra for months and years, which I have known to happen in the instance of Gonorrhœa ? If we refer to the very action of a sharp injection, which, from the sensibility of the Urethra whilst in an inflammatory state, has sometimes proved so very painful as to excite for a time more inflammation than had before been experienced from the venereal stimulus, we shall find that such increased inflammation, produced by the injection, will not be able to support itself long ; for unless the injection be repeated, the effect first produced by it will subside in a very short time, but the original inflammation excited by venereal virus will still continue, for having once commenced, it retains a power of long supporting its own irritation—much longer than any other stimulus can or does. And further, I shall remark, that in a recent case, if a sharp injection be repeated for some time, and if at the intervals during the use of it, the inflammation appear to have subsided, if the discharge be thickened, and if it approach nearer to mucus in a healthful state, yet if the use of the injection be

withdrawn before the discharge has totally ceased, and that for some days, the venereal stimulus will again recover its force, and will again produce all the inflammatory effects which it had produced before the injection had ever been thrown up. For if the venereal irritation be not completely extinguished, it retains a power of collecting again in force.

I think I have truly asserted, that the stimulus excited by venereal virus has a power of supporting itself beyond any other stimulus that can, from one application alone to the Urethra, be brought into competition with it. I shall endeavour also to impress upon the recollection, that the discharge from the Urethra is not the effect of any ulcer, that it is not produced from any other change in the condition of the Urethra, but inflammation on the membrane, and of the glands which are distributed throughout various parts of the membrane. Such being the plain matter of fact, if it were possible to a certainty at all times, by any remedy, to subdue this inflammation, the disease would terminate like any other inflammation; and the analogy which has been drawn by some, betwixt the inflammation excited by venereal virus, and any other inflammation which was not produced by virus, would be successfully confirmed. But this is not the fact. An inflammation excited by one act, and by a cause which was not venereal virus, unless it be constantly supported by additional acts, will readily yield to proper remedies applied for the removal of it.

It does not follow in reason, because mercury destroys the action of Lues Venerea on the constitution in general, that it should be a specific against the local action of venereal virus, when topically applied. As well might it be expected, because the pulvis antimonialis possesses the property of abating a fever, that by laying it on an inflamed part, it should possess the property of abating that also. Mercury possesses no chymical power of neutralizing venereal virus, if mercury be mixed with venereal virus abstracted from the constitution. The power which mercury possesses, is that of producing a change in the constitution, by which the venereal stimulus is extinguished. One poison prevails over the other, by superior predominant activity. But in the instance of Gonorrhœa, no such revolution takes place; the venereal virus is only acting upon a surface, by so far stimulating it as to increase its general secretion. The venereal virus is not absorbed into the constitution, and whilst the discharge continues, I believe, the absorption of it seldom takes place; for as the whole of the membrane might be considered to possess a secreting power, it is natural to conclude that the absorbents do not much prevail throughout it. That mercury is not a specific against venereal virus, merely as venereal virus, is evident from other instances besides Gonorrhœa; for if a Chancre be dressed with mercurial ointment, spread on lint, and laid upon it, and if there be not enough of the mercury absorbed, so as to act effec-

tually on the Chancre from within the constitution, that will not cure the Chancre. A Chancre which is disposed to heal by this application, will heal also by dry lint. But mercury cures the Chancre by its internal action, which destroys the venereal action. Mercury, from its action, brings the constitution into such a state, that the virus cannot act any longer, and becomes evanescent.

I shall now proceed to lay down as general rules as I possibly can for the cure of a Gonorrhœa, and assign my reasons for every particular, as near as I can. And first of all, I cannot too strongly enforce an early application of remedy, as soon as the disease is discovered; for experience tells us, it proceeds by degrees through the Urethra; and experience also tells us, that those who wash the parts, and keep them clean after every act of doubtful connection, seldom or never are infected at all; for if the fluid imparted in consequence of connection be washed away, nothing of course will follow. And this will be found as successful from any other detergent wash, as from a mercurial one. The best remedy I know for preventing the infection, is the act of urining, and washing the prepuce and glans repeatedly well, and wiping them during that act. But as this is not always at our command, it is necessary that a substitute should be found, and which can be had at will. A solution of the vitriolic fluid, the preparation of which I shall proceed to demonstrate, is the most successful. It should be

applied as soon as possible, before the moisture on the Penis is dry : the whole of the Penis should be also wiped dry, for a Chancre has sometimes appeared at the very root of it. It has been said by some that the preventive applications often fail—that they disappoint—that it does not follow a man is safe although there be neither Gonorrhœa or Chancre, for a Bubo is sometimes found to be the first symptom—and that sometimes Lues Venerea is discovered in the constitution, where there has been previously no local symptom. All this may be and is very true; but it has no weight, in my mind, against the use of preventives, for whether they be used or not, none of those symptoms, happen when they will, can be treated medically before they are discovered, nor can any infection be imparted through them before they have appeared ; for there must be fluid, venereal fluid, to infect, and that is the only medium of infection. But the preventive method might set aside every mode by which infection can take place, which otherwise would. For if the absorbents take up the venereal fluid, as it is lodged on the Penis, and thereby a bubo or lues is produced, so by washing away that fluid, both or either will be prevented. I am confident neither can be encouraged or promoted by it.

As soon as it appears that, from a suspected connection, a virulent Gonorrhœa is the consequence, there are two methods which present themselves for procuring a rational and more speedy cure, than if

it were left to cease spontaneously by the dilution of discharge which is excited by the virus—the one is by assisting that dilution by sedative injections, and the other is by exciting a stimulus which will suspend the former stimulus, and by repeating that excitement until the former stimulus be extinguished; for in all injections, if their intentions succeed, the success must be obtained by a repetition of them. The degree of inflammation excited by the virus must be our guide in the adoption of our system, and if we do gain time by the interference of topical applications, it must be by availing ourselves of the VARIETY OF TREATMENT WHICH IS INDICATED FROM THE APPEARANCE OF SYMPTOMS.

The following solution I have found to be very successful, in the first stage of a Gonorrhœa. The proportion must be considered according to the degree of inflammation, which will be expressed by the feeling of the patient. It should be thrown up six or seven times a day—it cannot be used too often. If the progress of the venereal inflammation be at a stand, and if the discharge be kindly yielded, there can be no doubt but the Gonorrhœa will soon be at an end, especially if this injection be applied, as I mean it should, early in the attack of the disease.

Dissolve blue vitriol in a sufficient quantity of spring water, precipitate the solution with a sufficient quantity of lixivium tartari, (which may be

known by the effervescence ceasing)—suffer it to separate, and pour off the clear liquor—then wash the precipitate with warm water, set it by to subside, decant the clear liquor, and repeat the process with fresh quantities of warm water, till it become insipid and tasteless, at least of the salt—then filter the solution, and reserve the precipitate. Dissolve as much sal volat. sal. ammon. in distilled water as it will take up, and filter it. Mix as much of the above precipitate with the filtered solution as it will dissolve, which reserve for use.

This will be found to be a very preferable injection. I do not mean to attribute to it any other effect, than that it possesses a predominant power of extinguishing the venereal stimulus, and can be used where there is a degree of inflammation with more security and success than any thing else. — I mean to offer it as a preparation which acts safe and effectual. I mean to be understood in saying, that if it be applied properly in time, it will prevent the Gonorrhœa from coming on at all; and if it be applied as soon as a Gonorrhœa is discovered, it will prevent a progress of every symptom, provided the patient takes care to inject often, and observes the strictest temperance. Five drops of the fluid to an ounce of water is the proportionate medium for injection. As a preventive, it may be made stronger to wash the parts externally; a dram of the solution

to half a pint of spring water, is about the proportion.

There is another stage of Gonorrhœa, which I think requires another consideration, and for the cure of which other methods are to be taken — I mean where the inflammation proceeds along the Urethra, in consequence of neglect — where, from tardiness in the application of the patient, the stimulus has pervaded the whole of the membrane. When this is the state of the disease, it is not found to be always safe to attempt removing the original stimulating cause by the substitution of another. The plan for abating the inflammation must be soothing. Constant injections of warm milk and water, with the application of the steam of hot water, and a moderate proportion of Mercury should be administered daily. Four grains of the Quick-silver Pill, with one grain of Opium, will not only be necessary to guard the constitution, but will in process of time so far affect the secretion of the mucous membrane, as to render it less exposed to the irritability of the virus. From the little danger there is of the constitution being liable to be infected, where the virus is only acting on the surface of a mucous membrane, and from the length of time it will take for mercury to produce its effect on this part, it cannot be presumed that the end obtained from such a quantity of mercury is to bring on a salivary affection. No such idea is meant to be inculcated. It is meant only to produce such an effect,

as the circulation of the mercury within the constitution will produce, without going all the length of stimulating the glands of mucous membranes. I have never known an excess of mercury abate an inflammation in the Urethra, and I have always experienced the discharge protracted, and the inflammation supported from a wanton continued use of mercury. I therefore consider mercury to be, in this instance, productive of good, by meliorating the disposition of the mucus, through a change which it procures in the action of the virus.

If in a reasonable time these symptoms do not abate, but on the other hand the ardor urinæ, chordee, increase, and the discharge becomes thinner, and mixed with blood, if a deep-seated pain be felt towards the anus, and the patient cannot sit down without expressing great uneasiness, still the soothing plan ought to be more rigorously pursued. Bleeding frequently by Leeches in the Perinæum, and the warm bath, or decoction of poppies; five grains of James's Powder should be given every night till the symptoms are abated, — the Mercury and Opium at the same time should be persisted in; and discretion will teach how these remedies are to be repeated.

Sometimes, in consequence of high inflammation, the membrane and glands will be so inflamed, that the discharge altogether will be checked. In this case, the method last laid down exacts our most earnest attention—for this, I look upon it, is the worst con-

dition of a Gonorrhœa. The origin of most of the obstructions in the Urethra is from this inflamed condition of parts. From the thickening of the corpus spongiosum a Chordée will be continued, even after the inflammation has subsided—from the stretch which it is put to in a state of erection, a hæmorrhage often comes on, and this I am never sorry to see, for it generally brings on favourable symptoms, by unloading the parts.

After the inflammation has subsided, by adopting the means already laid down, there will remain enough to be done for the consequences that are left behind it; for when once the whole of the Urethra becomes thus highly inflamed, it will often be a long time before an attempt can be made to put a stop to the disease by injection. And it is for this reason that, to treat a Gonorrhœa successfully, I have always recommended to the patient early applications, when injections will be of use, and when the same injections which would then produce a cure, will not be safe to be hazarded till some time after. The termination of the inflammatory state may be known by a return of a kindly discharge, which will be ropy and thick, leaving a daub upon the linen like white paint, and which, gradually becoming less and less, will cease altogether. The parts will again enjoy their natural functions, and then the disease will be at an end.

But as this is not always the case, when the parts have been severely attacked, I shall proceed to con-

sider what is the nature of the symptoms which are left behind, what are the ravages which the virulent stimulus has produced throughout the Urethra, and endeavour to follow up those consequences which do actually happen from this disease, although a Gonorrhœa has been said by some, and although it is believed by others, to cure itself.

On the contrary, so far from this disease curing itself, it appears to me to be one of the most intricate subjects either to be cured or rationally discussed. Glandular parts and secreting surfaces, once deranged, may never be restored — their organization is so delicate and fine, their orifices are so nice, their tubes and sphincters having once lost their functions, where is the power of art that can restore them?

After the disease has continued beyond the reasonable time, and when the inflammation has declined, if a discharge remain, it will be necessary to consider whether this discharge be continued by the positive action of venereal virus, or whether the parts have sustained an injury, and whether any particular diseased part, or indurated glands, support it. If the discharge be supported solely by a continuance of the virus, that will be discovered by the freedom of the stream of urine, by no particular part being affected, by the discharge being regularly distilled, and by the sensation which is felt being generally diffused along the Urethra: or it will be discovered, *a posteriori*, by the discharge kindly de-

clining, through the use of the vitriolic injection, or by the inflammation increasing upon ceasing to inject it. This condition of a Gonorrhœa is very often that which puzzles a Practitioner, and makes him fly to various and contradictory remedies. I shall therefore dwell a little upon it, and describe it as it generally happens: for, independent of all injury which has been done to particular parts, from no other cause than barely from that of the stimulus excited by remaining virus, the discharge will go on for a long time, and more especially so, if there were any neglect or omission, either at the commencement of the disease, or, what is not uncommon, during the period of the cure. I will explain what I mean, by stating a case.

A person applied to me for a Gonorrhœa, as soon as he discovered it. I gave him the injection, page 113, which answered very well, and in ten days he crossed the water to Boulogne. He took no injection with him, because he thought himself well. Whilst he was absent, an inflammation came on afresh, and doing nothing to abate it, before he could again reach me, the inflammation had gone to such a length, that all discharge was checked. I treated him after the manner I have already stated for such a condition, and then, by a return to the injection, he got quite well. Patients are apt to cease injecting immediately, when they find the discharge is at an end; but I always caution them to proceed many days beyond that point. For if the

injection which abates the symptoms be followed up, there is no doubt of success ; but success cannot be obtained unless it be regularly followed. — This is the state of the case in one instance. The state of the case in another is, where, although an injection does keep the inflammation under, yet whenever it be withdrawn for any length of time, the discharge will become acrid, and the inflammation will increase. I do not, in this case, change the injection ; I only increase its force, and repeat it oftener, which I find generally successful ; but if it should not succeed directly as long as all inflammation is kept down, I am not fearful of consequences ; I inculcate patience, and in time the cure takes place. I avoid, as much as possible, the change of remedies in this intricate and obstinate continuation of discharge, whilst I see the ground is safe, though the cure is slow. If I dishearten the patient by persevering in this manner, it would dishearten me, in foregoing my own reason, to oblige his conceit ; and I render him more obligation by adhering to this system, than by yielding to any other.

I therefore wish to impress upon the recollection, that the virulent inflammation will repeatedly return, after it has been partially extinguished, and that it is owing to the means which first brought the cure almost to a conclusion, being omitted—That a return to those means, where the violence of the inflammation does not forbid it, will again succeed, and that it must not, for some length of time, be

concluded the stimulating power of the virus is at an end, when an inflammation does return after the remedy has been withdrawn. I flatter myself this observation will demand some credit, will be found to be generally true, and will preserve those who give it attention from some miscarriages and some contractions in practice; for it must always be remembered, what it was which gave rise to the inflammation, and what it is which most probably is the support of it. This is a ground which should constantly be made good, before any conjectural cause be started for the continuance of the discharge, lest by leaving the true cause behind, a wrong scent be pursued, and the practice of error take place of the practice of reason.

An opportunity is now offered for me to make some observations upon the remedies which are in use for the cure of Gonorrhœa. And first of all, I shall speak upon the use and abuse of injections. If those who condemn the use of injections draw the line for fair argument, by asserting, that no injection, of any preparation, is proper to be thrown up the Urethra, during any stage of a Gonorrhœa, I should reply to them, that such assertion is not commendable, and that it is injurious to the advantages which are obtained by the contrary practice. If they admit that there are injections which are proper at the commencement, and at the close of the disease, which, on account of the inflammation, cannot be used with safety at the height of the dis-

ease, where, except milk and water, or what is as simple, nothing can be ventured by way of injection; in that case I perfectly agree with them. There cannot be a stronger proof of the superior advantage of injections, than their keeping the inflammation under; for as it is the inflammation continuing for a length of time, which only brings on the derangements in the Urethra, if that be shortened in its period, or lessened in its effect, if only this point singly were gained by injections, they would be of sufficient value to recommend and justify them. But more than this can be done by injections. Repeated injections can prevent the extension of the virus over the Urethra, by an early application of them, and save the parts from any inflammation; and injections can close the disease, when no human power by any other means can effect it. A gentleman of Plymouth sent to me his case: he had had a Gonorrhœa upon him for two years, had been salivated twice (this is strange to be told in the present time), had made use of the hot bath, had taken the balsams, gum Arabic, opium, and bark. I advised him to inject, and bathe in the sea; and in a fortnight I received a letter from him that he was well. From what I have already observed it will appear obvious, that it is not the use of injections which should be decryed, but the abuse of them. Those who object to injections, because they can do mischief, should be requested not to forget, that

every good in life might be equally proved to be an evil, through a perverse application of it.

I shall only make one more observation, for I am not fond of bringing forward a number of remedies, of shewing them on purpose to expose their inefficacy, or of reviving a catalogue of those remedies already rejected — it must be obvious that I know they have been in use, and that if I had selected them through choice, I should through choice produce them. But it might not be a waste of time to reflect, whether the ancient method of cure, by purging, has any preference or pretensions to be continued. If it be asked, during the inflammatory state of a Gonorrhœa, whether it be proper to keep the body cool, or whether from high living and a costive habit, a Gonorrhœa will not be protracted, I answer yes. But if a cure be undertaken and continued upon the purgative system, if a constant irritation be kept up, by repeated purgative medicines, I to that answer, no. There is no method of cure I have ever seen adopted which protracts a Gonorrhœa so long as this; and there is no method of cure which exposes the patient to an absorption of venereal virus more than this; a swelled testicle often comes on from this cause, a dysury yet oftener; and the discharge from this treatment will be spun out to a tedious length. I think upon the whole it is safer to do nothing than to attempt a cure by purging medicines.

I have already suggested what I think a necessary and prudential caution, that it should not be too soon taken for granted, the virus is extinct towards the close of a Gonorrhœa ; and I have endeavoured to make a distinction (both from the symptoms and from the effect which remedies have upon them) between the actual presence of virus, and the consequent effects of it. These consequent effects I shall now consider, for at present I am pursuing and defining the true nature of the discharge, and endeavouring to discriminate where infection ends, and where that which is termed a gleet commences.

If it were possible to discriminate with accuracy between a discharge produced from the mere habit of the parts, in consequence of former inflammation, and if it were known that the continuance of that discharge is not a necessary act in nature, in order that the thickening of the corpus spongiosum may go down, that the membranous surface may recover its former sound state, and that the induration of the testicle may all be dissolved—if it were possible to distinguish between a gleet, and a discharge which is a necessary act in nature to restore parts to a healthful state, we should be rewarded for our sagacity in knowing to a certainty that the one ought to be stopped, and the other let alone. This distinction is not so difficult but some clue can be given to assist us in making it out. If the patient be of a delicate habit, and of a pallid complexion, if he has practised an excess of venery, and is other-

wise debilitated, if the discharge be accompanied with pains in the loins, if during the Gonorrhœa a Chordée was not very troublesome, if there be no symptom of it remaining, if the testicle did not swell, if the stream of urine be large, but not strong, I should conclude that the discharge ought to be stopped. I look upon it, that as the constitution in this instance has some concern in the protraction of the discharge, so ought the treatment for the cure of it to be constitutionally considered. Bark and steel, the cold bath and the injection, p. 113, may be first of all tried ; if these do not succeed, the balsam copaiva should be taken. But all of them will not sometimes succeed.

ON CHORDE'E.

The Chordée in general goes off with the rest of the inflammatory symptoms, but it is sometimes experienced to be a continued symptom after inflammation has disappeared. I think but little of it in point of importance, nor should I now have dwelt upon the subject, had it not furnished a late Writer with the opportunity of misapplying a new term in Surgery. He says that the symptom of a Chordée is felt from what he calls adhesive inflammation. This expression of adhesive inflammation by him, is what I should express, an adhesion of parts in consequence of a degree of inflammation. But neither of these terms, if they mean the same thing, or if they do not, are applicable to the con-

dition of the corpus spongiosum, when the symptom of Chordée is felt ; for if it were produced from adhesion of parts, I should consider the consequence to be unalterably permanent. I should conceive there could not be any change from disease to health, when once there was a complete adhesion of parts. My reverence and admiration of the wonderful works of Nature are very high, but I cannot be persuaded that a part can be again restored to a state of perfect organization, when, for a length of time, it has been obliterated by adhesion in consequence of inflammation. A Chordée, from the thickness of the corpus spongiosum, will remain for some time after other inflammatory symptoms ; but if an hæmorrhage should ensue, or if the discharge continue, it will gradually go off, at least I never saw it remain unalterably fixed—I never saw a Chordée that time did not cure.

ON PHYMOSIS.

Phymosis I have described to be an affection of the prepuce as well from Gonorrhœa as from Chancre, and I have described it also to be a redundant collection of extravasated lymph, together with an enlargement of the lymphatic glands. This disease will require some patience and rest. The Penis should not be permitted to hang down, leeches should be applied to the part repeatedly, or blood should be drawn from the arm ; it should be fomented with a decoction of poppies, to which one

third of spirit of wine should be added, and a poultice should be applied with spirit of wine in it also. Four grains of the Quicksilver Pill should be taken every night, with a grain of opium. He should take every night five grains of James's Powder. Warm milk and water should be frequently thrown by a syringe between the Glans and Prepuce; and what is still more valuable, but never noticed, THE PATIENT SHOULD ALWAYS URINE WITH THE PENIS IN WARM WATER, TO WASH AWAY THE URINE CONFINED WITHIN THE SWOLLEN PREPUCE,—but more of this when I come to the treatment of PHYMOSIS, CONNECTED WITH CHANCER. A continuance of these means will certainly reduce the inflammation. To prevent a return of inflammation, and to brace up the part (for the vessels having been loaded, are liable to a topical congestion) the Penis should be frequently washed with liquor ammoniæ acetatæ, or with ammonia dissolved in water.

ON PARAPHYMOSIS.

Paraphymosis is a symptom of the same nature, but with aggravated circumstances. If the stricture be not taken off at a certain critical time, the glans Penis will mortify. The Paraphymosis is always at first a Phymosis. The prepuce has been at some time slipped back, and could not afterwards be slipped forward upon the glans, but remained in that state. If there be danger of mortification, an incision on either side of the Penis, through the

stricture, will relieve it, and in other respects it is to be treated as Phymosis.

I have already noticed upon the subject of gleet, that the discharge which was the effect of a relaxation of the whole of the parts, should, if possible, be removed by topical injections, and by a constitutional regimen. But this is not the only cause, as I have also remarked, for a long continued discharge from the Urethra. There are others by far more obstinate, which demand our attention to be separately investigated, and which after they are investigated will sometimes baffle our art. Diseases in no part of the human body are found so difficult to be discriminated and cured as mucous membranes and glandular bodies when deranged. Such cases become additionally intricate from their situation being in a narrow canal, remote from sight, especially as the soundness of that canal and parts contained within it, is important, and necessary to life itself.

CURE OF A SWELLED TESTICLE.

A Swelled Testicle has been already fully described by me, and I beg it to be considered that I am only treating here the Swelled Testicle in consequence of recent Gonorrhœa; I therefore shall proceed directly to the cure of it. The true method is very simple, and the consequences safe. The cure of a swelled testicle consists as much in forbearance from the former practice of some means,

as in the selection of others — as much in what we should not do, as in what we should. It is a complaint which exacts some patience, and some passive obedience to it. If it comes on at the same time that there is a discharge from the Urethra, the symptoms of it will be more moderate, and less will be required to be done for preventing or curing it. In this case, the patient should be in bed, suspend the testicle in a bag truss, and lay upon the testicle folded linen dipped in liquor ammoniæ acetatæ. No injection of any sort should be used at this time, nor should any other means be taken to stop the discharge from the Urethra.

If the discharge returns in full quantity, depend upon it the swelling of the testicle will not proceed to any alarming degree; but if the discharge does not return, and as long as no discharge returns, the testicle will continue to swell, and every symptom will be increased. In this situation of the case, bleeding in the arm, or leeches on the part, fomenting with decoction of poppies, to which spirit of wine should be added, poulticing, and suspending the testicle, and keeping constantly in bed, both with a view to the advantage of horizontal position, as well as to keeping the pores open, taking five grains of James's Powders, with a grain of opium once in the day, will all be necessary. A repetition of these means will abate the pain and inflammation, but if it should not, an emetic is sure to do good; and when they are abated, to prevent a re-

turn of them, to which, by any indiscretion, there will always be a propensity, and also to prevent a topical congestion on the part, from a relaxation in the dilated capacity of the vessels, linen should be dipped in liquor ammoniæ acetatæ, and laid on the testicle. This application should be frequently renewed.

If the swelling does return a second time, it is seldom equal to the former in point of violence, for the coat of the testicle, which is the seat of pain, having been once distended, complies with more readiness again than at first. Sometimes the testicle will come to abscess; but this is generally the result of neglect in the patient, or inexperience in the Surgeon — more often owing to the latter than the former — for the pain is so great, and the difficulty of going about so forbidding, that the patient, if both of these were necessary to a cure, would hardly submit to either.

But after all, a return of the discharge from the Urethra is of more importance, and more accessory to the cure of a swelled testicle, brought on from a venereal cause, than every medical art that can be devised. This simple act of Nature is superior to all the combined power of medical ability; and this act is cherished, by withholding injections and purging stimulating medicines; by keeping the patient in a horizontal position, and his pores open. This is the reason why a testicle which swells and inflames through venereal infection, more

rapidly than through any other provocation, subsides and recovers with more facility than from any other first provoking cause whatsoever.

CURE OF A GONORRHŒA IN WOMAN.

From the seat of a Gonorrhœa being more upon the surface of the parts in woman than in man, from the injuries done to parts by the effects of virus not being so serious or complicated in their nature, less is required to be said upon this subject; and from what has been already laid down for the treatment of the same complaint in man, less is therefore necessary to be said for that reason. All the superficial parts, and the Vagina throughout, are exposed to the action of the virus in a Gonorrhœa; and most of the symptoms which are experienced by a man, may, in like manner, be experienced by a woman.

In the beginning, the symptoms will be, a pain in making water, a tumid fulness of the labia and nymphæ, a soreness of the Vagina, and a plenteous discharge, the condition of which will be conformable to the degree of inflammation. For the cure of this stage of the disease, I should recommend the injection, p. 113, which I have already proposed, to be thrown up five or six times a day, and a pill of mercury, four grains, with a grain of opium, to be taken once in every twenty-four hours. It must not be forgotten, that in all those cases where the injection succeeds, it should not be discontinued too

soon ; it should be gone on with at least a week after every actual and every suspected symptom has disappeared.

But of this I am confident, that an early attention to consequences and a timely precaution will prevent the symptoms of a Gonorrhœa from ever coming on after an infectious connection. If the patient, after a suspicious connection, washes effectually all the superficial parts, and throws the injection a few times in the course of the first and second day up the Vagina, there will be nothing in future to be dreaded or cured.

When the symptoms which I have described are aggravated, and when the inflammation of parts is very extensive, the following means should be adopted. The parts should be fomented with a decoction of poppies, and the patient should be bled, take James's Powder in small proportions, dilute plentifully, continue in the bed chamber, urine in warm water, and the mercurial pill with opium should not be omitted. I have seen instances, from the violence of inflammation, where a sloughing of the Labia has taken place. As such consequences cannot happen but from the most palpable neglect, and when nothing has been done by way of remedy, the same means which I have already prescribed, only with the addition of some spirit of wine to the fomentation, and some to a poultice, will eventually succeed.

Sometimes the symptoms of a Gonorrhœa are not limited to the parts already described ; I have known

the affection to extend over the lower abdominal region, stretching round to the loins and hips. I have seen an inflammation from this cause equal to any that any other could produce. A gentleman applied to me for a Gonorrhœa, who from the little discharge that remained, fancied himself safe enough to lie with his wife, who was then residing sixty miles in the country. In consequence of the connection she was infected with a Gonorrhœa. The inflammation continued to increase more and more, until she experienced a great tension above the pubis, so that she could not bear the bedding which covered her to touch the part. The discharge was very thin and acrid, excoriating wherever it touched. He sent an express for me to come down to her, which I obeyed. I bled her often, fomented the parts, kept her body open, and gave her pulvis antimonialis with opium. When the inflammation had subsided, she used the injection, p. 113, took four grains of the mercurial pill with a grain of opium every night, which effected a cure. I saw another case in consequence of a Gonorrhœa, which I attributed to the affection of the same parts from venereal virus. This case also proved successful, from a similar treatment.

I think it necessary to observe, that whenever women apply to Surgeons for complaints seated in these parts, it becomes important to find out the true cause of complaint. It might be either Gonorrhœa or Chancre, or neither—it might be some

disease about the meatus urinarius—it might be an affection in the uterus, or it might be a Fluor Albus. There are many causes productive of similar sensations, which they, not seeing, can only describe from feeling, and which they, not conversant in, can only describe indefinitely. Prudence, and regard for our own reputation, ought to direct us to investigate those causes, and to obtain a true knowledge of them ; although with the most squeamish, or with the most modest, we may often find much difficulty of getting their acquiescence to an ocular inquiry.

A Fluor Albus may be in consequence of stimulus excited on the parts from venereal inflammation, or may be the remaining effects of virus. But here the same observations and the same precautions ought to be remembered, which were given in my former definite statement of Gonorrhœa and Gleet. Whilst any discharge is continued, in consequence of an original venereal stimulus, I cannot take upon me to fix any limited time when the power of infection ceases, before that discharge ceases also. I know that the man who constantly cohabits with a woman thus conditioned, may not feel any effect of the stimulus, when a fresh man will.

When a woman has been infected with a Gonorrhœa, who before such infection had no more discharge from the Vagina than that which was necessary to health, it behoves her to attend to her symptoms, until she be restored to that state again ; for

whatever discharge remains more than there was before she was infected, is to be suspected. The Fluor Albus is common to many women, who have never been in the way of contracting a Gonorrhœa. The constitution, the passions, the manner of living, have a share in a Fluor Albus which did not originate from a venereal cause. But this being foreign to my purpose, I shall return to the gleet remaining after Gonorrhœa — for the cure of which, I recommend the injection, cold bath, bark and steel.

In some cases of highly inflamed and protracted Gonorrhœa, strictures in the Urethra, diseases of the Prostate Gland, Bladder, and Kidnies, occur at remote periods. But these consequences form only one cause, where there are many other causes of serious and dangerous attacks on those parts. They are not considered necessarily to be treated as a part of Lues Venerea. For this reason I have abstained from describing them in this place. Whoever is desirous of the fullest information upon these diseases of stricture in the Urethra, and of affections of the Prostate Gland, the Bladder and the Kidnies, will find them already treated on at large by me, in two distinct publications :

1. A Critical Inquiry into the Ancient and Modern Methods of curing Diseases in the Urethra and Bladder. *Sixth Edition.*

2. Cases of the Successful Practice of Vesical Lotura, in the Cure of diseased Bladders. *Second Edition.*

CHAPTER VII.

ON CHANCER.

LUES VENEREA is proved to be the most difficult of all diseases to be discriminated. Cases are often suspected to be venereal when they are not really so, and cases are in the event found to be venereal, when they were not suspected to have been so. These mistakes cannot be said to arise out of our want of attention; for to speak generally, there is no one disease in particular which employs our attention more than the venereal; as every practitioner is under a daily necessity of furnishing the mind of his patient with some idea or other of the *Lues Venerea*, and therefore he is under the necessity of adjusting in his own mind some opinions upon it. It must necessarily be so, whilst he continues to practise; for if he should not inform himself through the pride of obtaining information, he must through the force of necessity; and if he be not a proficient from the love of science, he will become so from the passion of interest.

A Chancre is to be considered as a *prima ordo* of venereal infection. It is sought for and found on the glans Penis, behind the glans Penis, upon the

frœnum, upon the internal duplicature of the prepuce, or externally upon any part of the Penis. As the most general mode of conveying the venereal poison is by a venereal intercourse between the sexes, so are the effects of that intercourse, when either any part of the penis in man, or labia, vagina, or nymphæ, in woman, possess venereal poison, imparted from the one to the other. It is by the intercourse of a sound person with a diseased, that the infecting principle is constantly supplied, and supported. .

The infecting fluid must be parted with, and left to act undisturbed, in order to infect a sound person; for without the medium of fluid, it has not been found that infection can be imparted. Venereal poison does not consist of those fine, light, and volatile particles, which may be dispersed in air, be carried at a distance; or will speedily pervade the pores; but in order to take effect, it must be by a fluid left to go on in an uninterrupted process. On whatever part of the skin this fluid be lodged, it will there produce a Chancre. The skin of the Penis is as much defended from infection as any other part of the body, and so is the scrotum which is annexed to the Penis, and nearest to it; but these will not be found to be exempt from the action of venereal fluid, as Chancres are not only constantly seen on the external part of the Penis, but on that part of the scrotum also nearest to the Penis. Now it is evident, there is not that difference between the

skin on the Penis and the skin on any other part of the body, which will induce the fluid to act upon the one, and prevent it from acting upon the other : it must therefore be obvious, that if any other part of the skin were in the way of receiving that infection which operates upon the skin of the Penis when it is received there, it would have likewise operated upon that other part.

∴ The glans Penis may be considered as a part in anatomy similar to the internal membrane of the Urethra. The parts behind the glans Penis are replete with small secreting glands, which discharge a sebaceous substance, and this is generally affected according to the state of health ; sometimes it will be thin and acrid, at others it will be redundant without acrimony ; and sometimes there will be a soreness, with inflammation. The frænum is a portion of the prepuce annexed to the glans. This and the whole of the internal part of the prepuce, which, in the natural pendulous state of the Penis, serves as a covering over the glans, are both either devoid of cutis, or it is evidently thinner on them than elsewhere.

As it so happens that the Penis, which is a part exposed to the first act of venereal infection, is a member made up of various constructions in anatomy, so must the venereal symptoms be absolutely different when the virus is lodged on any part of it ; for it must, when it operates, produce symptoms of its action peculiar to that part. Thus will the virus

received from another, if lodged only within the commissures of the Penis, produce a Gonorrhœa, when the same virus, lodged behind the glans, or under the frœnum, will produce a Chancre : and thus will the same virus, when lodged withinside of the prepuce, or under the frœnum, or behind the glans, produce a Chancre in a much shorter time than when the same virus lodged on the glans, the external part of the scrotum, or on any other perfect cutaneous part, will produce a Chancre there. The glans Penis appears to me to be a part difficult to be infected, and I draw my conclusion from seeing that it is not so often the seat of Chancre as other parts equally exposed, but which cannot be more so. The appearance of Chancre most in common is either under the frœnum, or behind the corona glandis ; the next in common is withinside of the prepuce ; and the rarest appearance is on the external part of the Penis, on parts adjoining to the Penis, on the corona glandis, and every other part of the glans. Sometimes a Chancre will commence behind the corona glandis, and spread upon the substance of the glans ; but if the glans be not affected after this manner (and from its contiguity to a part more liable to receive the impression of infection, this may be a cause of its being infected) it will seldom happen that it will be infected at all ; although it be in the way of being most affected, as I before observed.

On all these parts, when affected by venereal fluid, the affection will be a Chancre; but if the Chancre be situated behind the frœnum, it will seldom there excite more discharge or inflammation than what may be said to be in consequence of the Chancre itself. If it be situated behind the glans, amongst the sebaceous glands, or withinside of the prepuce, the former situation will provoke an additional discharge from other parts, the same in their texture, and near to the original seat of infection; and the latter will be swelled, become hard, and thickened, sometimes throughout. When a Chancre is situated on the skin of any part of the Penis, or at the root of the Penis, or about or on the scrotum, the inflammation and thickening of the parts are most commonly confined to the seat of the Chancre itself. But whenever a Chancre appears, and on whatever part it appears, the strongest criterion of a true venereal Chancre does not consist so much in a separate and distinct ocular appearance of a palpable ulcer, as it does in that certain thickening of the parts which, soon after the first appearance of it, is always to be felt. I am so much guided by the feel of the surrounding parts, in my decision, whenever I am to define a suspected ulcer to be a Chancre, or not, that if my reputation were at a stake upon a right decision, I would rather trust to my fingers than to my eyes, provided I had my choice of either, but was debarred from the use of both.

I have already observed, that the original symptoms of venereal infection, when it was first discovered in Europe, were not at all changed by time; that the symptoms will always be the same, although an accurate discrimination of them, as the same venereal fluid affected different parts, might not have been so successfully made by our ancestors from want of experience, as they can now be made by us. A discrimination of symptoms could never have been made with any success at any time, if the subject which is attempted to be discriminated were not subservient to one general rule, or universal law. The symptoms, therefore, of this disease, or any other, will be the same, as well before we are able to discriminate them, as they will be after. Although discrimination of symptoms cannot alter the symptoms, yet it will not only assist us to understand them, but also, where it be possible, assist us to cure them, or prepare us to submit to the event of such as cannot be cured.

In the year 1494, the process of a Chancre is thus described by MARCELLUS GUMANUS: "I observed," says he, "many of the Officers and foot soldiers at Milan, whilst I was in the camp at Navarre, to have several scabs or pustules breaking out on the face, and spreading all over the rest of their bodies; the first of which appeared usually under the preputium, or on the outside, like a grain of millet; sometimes behind the glans, with a small itching; at other times a single pustule would arise,

like a little bladder, without much pain, but itching also. If rubbed or scratched, there arises an ulcer, corrosive, and something like the sting of an ant."

VIGO, in 1513, describes them thus: "The Venereal Disease has its origin in the genital parts, namely, in the Vulva of women, and Penis of men. There almost always appear small pustules, sometimes of a livid colour, sometimes black, and sometimes whitish, with a CALLOSITY surrounding them."

I think these quotations sufficiently prove, by indubitable testimony, that the original symptoms of pustule or Chancre were just the same at first as they are now; and it does not appear that ASTRUC, who knew more of Lues Venerea than any Author which is upon record, and who marked, with the exactest accuracy, every particular, has doubted in the least but the symptoms at the origin of this disease were the same then as when he described them: and his description will serve to confirm my position.

"Chancrous ulcers, from impure venery, are small superficial ulcers, very little hollow, round, callous, and obstinate, occasioned by the venereal poison, which breaks out upon the pudenda. They are situated, in either sex, upon different parts of the genitals; in men, frequently upon the internal surface of the prepuce, and upon the back or the sides of the glans; most frequently upon the corona or frænum of the glans; sometimes, too, upon the extremity of the duct of the Urethra: in women, oftentimes upon the internal surface of the labia

pudenda, very frequently upon the clitoris, the root of the clitoris, or on the prepuce with which the clitoris is covered ; upon the nymphæ, carunculæ myrtiformes, and Vagina ; nay, sometimes upon the extremity of the Urethra, near the lacunæ. At first there arises a small miliary, red, pointed, hot, itching pimple, the top of which, by degrees, grows whiter, and more even, and at last opens with a small aperture, from whence a small quantity of ichor is discharged. The erosion of the lips continuing, the ulcer increases, is dilated, and forms a small cavity ; IT IS SURROUNDED WITH CALLOSITIES, MORE OR LESS HARD AND THICK ; it abounds with a thick, viscid, tenacious pus. Ulcers of this kind differ ; first, in situation ; for some belong to the glans, as those which occupy its back or convex part, or, which is more frequent, the corona ; others to the prepuce, as those which cover its internal surface, or its extremity ; lastly, others belong to both, as those which are situated here and there upon the frænum, by which the prepuce is connected to the interior part of the glans. Sometimes they are few and distinct, sometimes numerous and confluent ; sometimes they are crowded together, and collected in one area ; sometimes they surround, with a circular ring, the corona glandis, or the roots of the prepuce. Some are of the benign kind, which are round, superficial, very little callous, white at the bottom, discharge a laudable pus, and have lips which are neither red nor enlarged ; others,

on the contrary, are malignant, of an irregular angular figure, with a black, livid, dark, purplish-coloured cavity, have hard, callous, prominent, red, inflamed lips, discharge matter rather of an ichorous kind than pus, and spread deep and wide."

Thus it appears, that every part of the Penis is exposed to the action of venereal virus. The malignancy or the mildness of chancrous ulcers will depend, in the first instance, upon their situation: and in the second, upon the condition of the parts. If a Chancre be seated either upon the frænum, upon the internal surface of the prepuce, upon the glans, or upon the external cuticular part of the Penis, the inflammation will not be so general as if it be seated behind the corona glandis; for if the sebaceous secretions there be fœtid, and out of condition, previous to the appearance of a Chancre, it will be provoked to appear with more inflammation, with more thickening of the parts, and with more malignancy and obstinacy, than if these parts were in good condition. The next part where a Chancre is seated, which is also susceptible of additional irritation, is on the internal surface of the prepuce, and this from experience we find will produce an inflammation so extensive as to prevent its being slid back, when it is forward; and from being slid forward, when it is back.

It very often happens that the sebaceous glands yield their secretions so much out of condition, as to inflame every part adjoining to them, namely, the

glans and the prepuce, insomuch, that such an appearance may be taken for venereal infection. But although there will be an excoriation and inflammation, in consequence of this state of parts, and sometimes a thickening of the prepuce, so that it can with difficulty be slid back, yet after cleaning the parts, no ulcers will be seen similar to chancrous; the inflammation will be more universal from this cause; but by cool diet, and by washing the parts with some mild astringent frequently, the whole will disappear.

The date of appearance of Chancres on these various parts, from the same contraction, will be different. For the internal surface will be more readily acted upon by venereal virus, than the external; and the virus will more likely be lodged on each side of the frœnum, and behind the glans—will more likely remain undisturbed within and about the cryptæ of the sebaceous glands, than upon the glans, or upon the external parts of the prepuce and of the Penis. But if, from the same contraction, the virus should find a lodgment on the frœnum and about it, behind the glans, upon the internal surface of the prepuce, and upon the external surface of the Penis, and if the virus should remain on these parts, so as to produce Chancres, they would not appear all at the same time. The first appearance would be behind the frœnum, behind the glans, and upon the internal surface of the prepuce. The appearance of those on the glans,

and on the external part of the Penis, will not be until some time after. Chancres, situated behind the glans, are not so dangerous to infect the constitution from the absorption of virus, as Chancres situated on any other part; and Chancres situated on the skin of the Penis, have, in general, infected the constitution at the very time they are first detected. I scarcely ever saw a Chancre on the external part of the Penis, which did not, in spite of mercury, terminate in a bubo; for the inguinal glands have been inflamed at the moment of time the Chancre is first felt, or the virus has past them unmolested, and is already acting upon the constitution.

With respect to the distance of time from receiving the infection to the appearance of Chancres, it is too vague to be regularly ascertained, or for any general rule to be made out of it. If the Penis, on which the virus acts, be exceedingly dry, and all parts of it, when in a flaccid state, appear palish, the virus will not be found so rapid in its action on one thus conditioned, as it will on a Penis disposed to a redundant exudation of acrid secretion, and which looks red and tumid. The appearance of Chancres after connection, in the former instance, will be six or seven days, or later, on those parts which favour the earliest appearance; and perhaps three weeks or a month, on other parts; and the appearance of Chancres, after connection, in the latter instance, will be in a day or two, on parts most susceptible of

infection. But when a Chancre is detected on the external skin of a Penis, where the internal surface of its prepuce and its sebaceous glands yield a redundant portion of acrid discharge, I will not take upon me to say that a Chancre so situated, will from that cause become more malignant than on the skin of a Penis of the former description.

DEFINITION OF CHANCRES.

I shall state the condition and appearance of Chancre, under three separate heads ; but it must not be forgotten, that the difference in the appearance of Chancre is never owing to a difference in the quality of the virus which produces it, for the virus is always the same, but to the length of time that it has acted, to the difference of parts on which it is acting, and to the condition of the natural health of the parts. When an incipient Chancre is detected on the frœnum, as soon as it appears, it will be a little glassy acute sore, smooth and even with the rest of the surface ; it will be very sensible of a smarting pain upon being rubbed, or from slipping back the prepuce. And when it is detected behind the corona glandis, at its first appearance, it will be found to have excoriated the thin covering of the part where it is situated, and will appear also either shining, and in a small circumscribed circle, or it will inflame at some distance around it. A Chancre, thus seated, will not be so painful as the former. Sometimes

there will be many of them appear, at first very distinct, which will in time run into each other, just as though the virus received, from uncleanness, had never been washed off, but had acted upon the cryptæ of the sebaceous glands, and had ulcerated the whole of them. When a Chancre is found upon the internal part of the prepuce, it will there appear as a little pustule, such as I have described in my quotation from former Authors. The cause of its appearance always as a pustule, containing a watery fluid, is, that the thin cuticle which covers the internal part of the prepuce is yet thicker than on the frænum and behind the glans, but nothing like so thick and substantial as the cuticle on the external part of the Penis, but yet thick enough not to be destroyed before a Chancre upon it may be detected. At the first appearance of a Chancre there, it will be found to itch and to smart, and when the cuticle is destroyed, an ulcer will be seen, and the surrounding part will be somewhat thickened.

The first appearance of Chancre, both on the external part of the Penis, as well as on the glans Penis, will plainly point out that which I am endeavouring now to make appear, namely, THAT ONE DESCRIPTION WILL NOT ANSWER FOR THE APPEARANCE OF CHANCRE ON EVERY PART, IT WILL ONLY ANSWER FOR THE APPEARANCE OF EVERY CHANCRE ON PARTS OF THE SAME NATURE ; AND STILL THERE WILL BE A REFERENCE TO THE PREVIOUS CONDITION OF THE NATURAL HEALTH OF THE SAME PARTS. When a

Chancre appears on the skin of the Penis, it will be a red, hard, round pimple, with streaks of red leading from it; it will be in this state for some time, and the parts about it will appear at first thickened, but will harden also by degrees. It will sometimes not open, and yield a discharge for a considerable time after it be detected, and I have known it to remain in this indolent state for more than a month. When it does discharge, it will be a thin ichor, and it will be found to shew a strong disposition to ulcerate, and the parts around will inflame and redden. It is neither so smarting as a Chancre on the internal part of the prepuce or on the frænum, and nothing like so rapid in its progress as those which I have before described. A Chancre on the glans Penis, considering how much that part is exposed to infection, is very rare; I mean a Chancre not connected with the prepuce, but one which arises solely out of the glans. The cuticle of it will be raised, and a fluid will be discovered under it, as if it were confined within a bladder; and when this membrane is broken, and the fluid discharged, there will be a red spot remaining, which will in process of time become an ulcer, but it appears to be very slow in its action. The external orifice of the Urethra will be sometimes the seat of a Chancre, and which is also very slow in its action. But Chancres which are found on the corona glandis, are generally chancrous spreadings of venereal ulcers, which began behind the glans, and therefore their origin belongs not to

the corona glandis, but to the sebaceous parts behind the glans.

If the progressive course of Chancres be left uninterrupted, some parts will be sooner destroyed than others, and some concomitant symptoms of an alarming nature will be found to await on Chancres in one situation, that cannot happen from Chancres in another, although they may be all contracted at the same time, and although the predisposing constitutional affection be exactly equal in its influence on all. No person who reflects for a moment, can fail of seeing a difference in the effects of a Chancre proceeding to destroy parts about the corona glandis, and behind the corona glandis, and in the effects of a Chancre proceeding to destroy internal parts of the prepuce only, or in the effects of a Chancre proceeding to destroy the frænum, or in the effects of a Chancre proceeding to destroy the covering skin of the Penis. The action of the first must always be considered not only more serious, as relative to the preservation of the member itself, but the life of the patient may be sometimes at hazard; for if the Chancre has began mild, because the parts on which it is continuing its action, were in a sound state, yet the progress of it will only be lengthened from that cause, it will still proceed, and find its way into the corpora cavernosa, the corpus spongiosum, and the Urethra: so that it will not only destroy the Penis, but in its destruction sometimes an hæmorrhage will be experienced, which will endanger the immediate life of the patient.

This will be the consequence of virus left to act on this part, if the whole of the parts were previously in a healthful condition, but if the parts were previously not in a healthful condition, the symptoms of venereal infection would be proportionably more rapid, more aggravated, and more dangerous, for a mortification might ensue, and the whole substance of the Penis might be destroyed, before any assistance could be brought through medical aid to release it from the ravage of the virus, when connected with a previous constitutional derangement of the parts.

It necessarily follows, where a Chancre on one part will produce all its worst effects, that if it had fallen on another part, the worst effects would have been produced by it there also. But if a Chancre be situated on the internal part of the prepuce, it will only destroy that loose substance; or if it proceed to greater lengths, the whole of the prepuce might be spared without a feel for the loss of it; and the frænum also might be spared: if it proceed to destroy the under part of the glans, and to penetrate into the Urethra, that inconvenience may be compounded with; if it proceed to destroy the skin of the Penis, it will there be like an ulcer which is venereal on any other part of the skin. The danger from an ulcer thus situated does not consist in the mischief it can locally produce, but in the inevitable power there is, through it, of the virus being conveyed to the inguinal glands and into the constitution. These being not only the partial effects of particular Chancres act-

ing on particular parts, but also the general effects of all Chancres acting on every part, I shall just remark, that before they arrive to this stage, they will be found to be acting progressively as Chancres, different from each other on different parts, but similar always in their consequences on similar parts.

In the progression of Chancres situated behind the glans, the appearance of them will be like an honey-comb: and if the same Chancre spread, so as to extend to the corona glandis, which is contiguous to it, the appearances on the one and on the other will be widely different; for that part of the Chancre situated on the corona glandis will not be so eaten down as the other, and will be apthous at its base, the edges of it will bleed, and they will not thicken like the edges on the sebaceous parts. When the white apthous appearance at the base becomes clean, the cavity, which is made by the destruction of the Chancre, does not fill up, but the loss will be always apparent. In the progress of ulceration, the *frœnum* will be often eaten away: but that which is destroyed of the prepuce will be again supplied; unless it be of very great extent, and then the prepuce will become so contracted, as to continue to be a stricture on the glans. An ulcer on this part is seldom or never apthous, but of an honey-comb nature, and eating deep into the surface; and so will an ulcer also on the external skin of the Penis.

There is one symptom of a venereal ulcer, which

is common to all, and which is particular to a venereal ulcer, namely, that it will be surrounded by a thickening of the parts, and as long as this thickening remains, so long is the ulcer proceeding to destroy the parts, or the virus is proceeding into the constitution by absorption. Although it be necessary to treat on the whole of the consequences that may happen from venereal Chancres, and although it be necessary to ascertain the utmost local devastation that may be committed by them, yet these appearances do not always occur; when they do, they bring with them enough to alarm us for the consequences, and enough to excite us to employ our attention to guard against them. But this most general symptom of a thickening and induration of the surrounding parts must ever excite our attention, as an object which refers to future constitutional infection; for if the Chancre heal, and a hardness remain, it will either break out again, when the constitution becomes infected, or the hardness will still be increased, as ominous and indicative of a constitutional infection. This symptom, therefore, will always explain, by its presence, that the local infection is not radically removed; and by its absence, that it is. If by embracing the part which was the seat of the Chancre, the appearance be thin, so that the finger and thumb do almost meet, the cure may then be concluded to be perfect; but if a hardness and thickness remain, although it be healed, and if there be a scale upon the part where the Chancre

was, then the case must be deemed as not cured, and as requiring much more to be done for it. This observation cannot be too closely attended to; for, depend upon it, a Chancre will sometimes heal, and yet the virus will not be extinguished.

During the inflammatory state of the glans and prepuce, which is often excited by Chancres, both these parts, as also the frænum, will be subject to hydatids, or crystallines.—The blood vessels, in this instance, are not only turgid, but also the lymphatics become so beyond measure from obstructing lymph, and therefore the lymph will forcibly transude the over-distended vessels, and be collected under the cuticle, in one or more hydatids, or crystalline vesicles, prominent, broad, or narrow, just in proportion to the quantity of lymph collected. These vesicles will be filled with a fluid, thin or thick, pellucid or opaque, according to the quality of that which transudes. If the vesicles remain long entire, at length you will find them become flatulent; for the airy bubbles which are blended with the transuded lymph, as in all other fluids, being by the continued force of the inflammatory heat by degrees let loose, will occupy the space of the lymphous fluid which is constantly and gradually evaporating. Every injury done to the prepuce is observed to produce, in some measure, a redundancy of accumulated lymph.

Sometimes, in consequence of high inflammation, and at other times in consequence of former Chan-

cre, when inflammation has not been high, and even after as much mercury has been thrown into the constitution as can be warranted by the warmest expectations, there will yet remain hard chords, which are indolent and uninflamable. At length they may or may not inflame, become painful, and discover signs of a malignant disposition.—These will be found upon the *frœnum*, behind the glans, upon the pendulous part of the prepuce, upon any external part of the Penis, and also upon the scrotum. When not inflamed, they will bear touching, and will be found as hard as a dried pea. If they cannot be discussed, they should all be extirpated before they discover signs of malignancy.

DEFINITION OF WARTS.

Warts or *porri* are generally found behind the glans, upon the glans, about the *frœnum*, and sometimes within the internal part of the prepuce; but their chief seat is behind the glans, and upon the glans, most commonly beginning behind the glans. They spring up in consequence of porous exudation, and are always the effect of a redundant exudation from the sebaceous glands. They are animal mushrooms, and will be found to grow out of these parts, independent of any venereal infection. The same cause which now and then produces a change in the condition of the sebaceous secretions, by which the whole of the parts will partake of a temporary in-

inflammation, when there are no warts, will, by such a change in the condition of the sebaceous secretions, produce a temporary inflammation when there are warts, and then their condition will be aggravating, and productive of much fœtor, pain, and inconvenience; for the warts will become moist, will harbour fluid, will increase and turn into a bushy appearance, and will yield a nauseous discharge of fœtid mucus, mixed with blood—they will bleed at the touch, cause the prepuce to be slid back with pain and difficulty, almost deprive the possessor of venereal intercourse, and will, to a certainty, absorb any moisture that may come in the way of them.

If they be not originally venereal, they will be sure to become so, whenever a chance of contracting the disease readily offers. I do not know a more difficult, more nice, and more critical state of a case than this, *prima facie* to be truly decided upon. For a redundant exudation, and that exudation vitiated from any cause, will certainly produce these effects independent of virus; and the linen will be as much stained at times, as if the patient had a virulent Gonorrhœa, in its worst stage. And also when the warts were previously dry, and when they were supported by a little stem, after they have imbibed venereal virus, they will, from that cause, degenerate into a virulent state. They will also, from either a venereal cause, or any other, continue thus for a long while, appearing better by dry repellents, worse from an omission of them, and from high living;

for if they be supported by a venereal cause, as long as they continue to yield so plenteous a discharge, absorption of virus will not be so rapid in its progress, no more than it will from a Gonorrhœa in the Urethra; but these effects will continue to become more and more troublesome, as I have seen, for twelve months at least, and no other venereal symptom has arisen out of them; notwithstanding, at length, they have been cured through mercurial friction, when nothing besides could effect it.

I am convinced, it is owing to a plenteous discharge, that, in these cases, other venereal symptoms, when warts are venereal, do not so readily appear. As long as a discharge continues, unchecked by repellents, and as long as no mercury has been at the same time administered, I do not think other symptoms will appear; for whenever absorption does take place, either from this original cause, or from a Chancre, the discharge from either will no longer be yielded with freedom. In a Chancre the discharge will be considerably baulked, the Chancre will remain stationary, hard, and dry, and if it spread, there will not be a proportionate discharge to its increasing size; or if it heal, the process is never found to be kindly; there will be an ungracious malignant hardness remaining, and a short time will make apparent and confirm the lurking suspicions which such insidious symptoms excite.

I have already stated, that the distance, in point of time, of the appearance of Chancres upon parts,

when they are all contracted from one connection, will be according to the nature of the parts on which they appear, and on which the process of them is proceeding; I have stated, that the coverings of some parts are thinner than the coverings of others; and that the thinner the covering of any part is, the sooner will virus lodging upon it, destroy it, thereby producing an earlier Chancre, with the exception of glans, and that can be accounted for by knowing that the glans is a mucous surface, similar to the Urethra: I have also stated, that parts concealed, and parts the least in the way of being rubbed upon, are parts the most liable to harbour and entertain uninterruptedly the venereal fluid, so that its action will be more generally found, to a greater certainty, in a shorter space of time, and in a more multiplied degree, on such parts, than where the fluid has found a lodgment by accident; and where, when it is found to act, it is also proved to be an uncommon occurrence, and which is to be considered as a circumstance out of the ordinary way in expectation.

When we find many Chancres surrounding the parts behind the corona glandis, we see the natural consequences which may be expected from a promiscuous connection, and from an omission of washing them; and even when those parts are washed, we often afterwards detect a lurking Chancre behind the frænum, because the washing was slovenly done. Some virus might have been left also upon the skin of the Penis, and which, when

it be not washed away, will lay the foundation for a future Chancre there. But if from the same connection, there should be virus lodged behind the glans, and virus lodged also upon the skin of the Penis, and if the virus act effectually on both these parts, I will take upon me to say there will be a distance of many days between their appearance; for that on the Penis will be long after that behind the glans, before it be discovered, if the eye could be possibly as intent upon the part where a Chancre will be, as it can where it has already been detected.

I have before stated, that the first local attacks of venereal virus, are only so far confined to the Penis in man, and the privities in woman, as the usual mode of contracting and carrying on the infection is through that channel. But there are other channels by which the infection will find its way from one subject to another, for it is the local mode of infection that I am proceeding to treat upon.

It must not be forgotten, that venereal fluid received from another subject, will produce local venereal effects upon other parts besides these of generation; that important truth is too obvious, too commonly known, to be considered a new truth, and too well recorded in conjunction with a train of such continued, severe, and exacerbated symptoms as cannot otherwise be found throughout the whole of the history of cases, which have been given of the progress of Lues Venerea.

My present purpose is to explain some important theoretic points in the nature of this disease, namely, —**THAT VENEREAL FLUID PRODUCED THROUGH INFECTION IMPARTED BY ANOTHER SUBJECT, WILL BE HARMLESS TO THE SUBJECT WHO SECRETES IT. THAT VENEREAL FLUID THUS PRODUCED, NEITHER GENERALLY NOR PARTICULARLY, WILL EVER ACT TO THE PREJUDICE OF THAT SUBJECT WHOSE SECRETION IT IS ; WHEN IT APPEARS AND WHEN IT IS KNOWN, THAT VENEREAL FLUID OF ONE SUBJECT BY BEING IMPARTED TO ANOTHER SUBJECT, AND BY THERE REMAINING SO THAT SUCH FLUID MAY TAKE EFFECT, IS THE ONLY MEANS THROUGH WHICH THE VENEREAL DISEASE CAN BE PROPAGATED, AND CAN BE SUPPORTED.**

I shall, for the further elucidation of what I am hereafter to offer, make a few observations upon the liability of every part to receive a local infection of Lues Venerea by other means than venereal intercourse betwixt the sexes, when by any extraordinary manner the venereal fluid of one subject is conveyed, and left to act upon another.

In the rare case which I gave of a Gonorrhœa of the eye, and where the eye was lost *, it evidently appeared that it arose from accident, and that it was produced through the application of venereal fluid of another subject to that part ; for the subject who lost his eye through it, had knowledge of a woman

* Page 98.

half an hour before he went home, and when he came home, washing his eyes, because they were weak, as he had been in the habit of doing, with his own urine, he by that means imparted to his eye some of that venereal fluid which was imparted to him just before by the infected woman. The eye began to inflame before the venereal stimulus was discovered in the Urethra, and each Gonorrhœa after kept its own course: and although both of his eyes were washed, as both were weak, yet only one of them became thus infected, because none of the fluid of the woman, by being all consumed on the eye infected, or by some other failure in the action of the venereal fluid, took effect on the other eye*.

Venereal fluid, imparted from another subject, has the power of producing a Chancre upon the skin; and when it is there lodged, and remains uninterrupted, it does produce it. In general, after examining women, or delivering women who have a Chancre, or a Gonorrhœa, the hands are washed: it does not follow, therefore, that that which is possible to happen will happen, because the hands are

* I read in a publication of the late Mr. Ware, that in a patient's case of Egyptian Ophthalmia, he examined him to see if he had a Gonorrhœa Penis, to discover if he had infected his eye in that way. Mr. Ware, who entertained such a theory, and every body else who does, ought to know, without being told, that boys and girls who have a Gonorrhœa make use of their handkerchiefs indiscriminately, and that they never infect their own eyes from their own venereal fluid.

generally washed, and the adhering virus is by this act removed. But when there has been a cut or a sore upon the finger, in that case, notwithstanding the washing of the hands, and notwithstanding the insignificance of the cut or sore, for if it were important, it would certainly be from that cause bound up, the subtilty of the fluid has taken in many instances effect, a Chancre has been produced in consequence, and that Chancre has been seldom known to be cured, without a more decisive action of mercury, than where the infection hath been contracted in the usual manner, and on the usual parts. I know four practitioners in midwifery who have experienced this condition, and the case of one of them became so desperate as to baffle all art for nearly seven years ; when, during that time, all the medical men of this and other countries were employed to it.

A tooth in transplantation, which was selected for its beauty, and for its soundness, and which to the individual who first possessed it, was to all intents harmless in its original situation, and which, although the subject who originally possessed it was venereally infected, would have never, as a venereal tooth, been productive of ill effects to that infected subject who was the original possessor ; yet by this same tooth being transplanted into the socket of another subject, the local venereal infection was immediately spread, a general ulceration of the surrounding parts did follow, and Lues Venerea was sooner constitutionally

imparted in this instance also, than in the common and ordinary mode of its being communicated *.

I mean to infer from what I have advanced, that by the local application of the infected fluid of one subject so remaining upon another as to have an opportunity of taking effect, it will always take effect on any part—that it will from the law of poisons be a necessary consequence. And I mean to infer, as I shall explain hereafter, that the local fluid of Gonorrhœa or Chancre of a subject thus only locally infected either with the one or with the other, will not nor cannot venereally infect any other part, to which it can be applied of the same subject. In other words, I mean to say, that venereal fluid, so far as it refers to the same subject, and so far as it may be suspected to be capable of acting upon, and of producing either Chancre or Gonorrhœa, or both, when conveyed to another part of the same subject, in such a manner that it would, if it could produce them, will not produce them.

GONORRHŒAL FLUID ON THE SAME SUBJECT, SECRETED BY THE SAME SUBJECT, NEVER PRODUCES A CHANCRE ON THE SAME SUBJECT; AND CHANCROUS FLUID ON THE SAME SUBJECT, SECRETED BY THE SAME SUBJECT, NEVER PRODUCES A GONORRHŒA.

Those who from constant observation have declared, that the original cause which produced a Go-

* Sir William Watson gave a strong case of this sort in the London Medical Transactions. See also "Observations on the New Opinions of John Hunter, Part III. pp. 174—205.

norrhœa and Chancre was not the same, were led to this opinion from observing, if they were produced by the same virus, the natural consequences, in their opinions, must have been, that chancreous fluid, when it was within contact of the Urethra, would generate Gonorrhœa ; and that the continued lodgment of fluid from a Gonorrhœa upon the glans, withinside of the prepuce, and around the corona glandis, must to a certainty produce at least those symptoms which are found to be produced from one single connection held with a venereal subject, so locally diseased as to impart them. The fact is certain, that the one does not arise out of the other ; but the manner in which they attempted to account for it was not the true one. Their observation, that one symptom did not arise out of another upon the same subject, was right ; but their denial, that the first cause of the two symptoms were the same, that they originated from the same fluid, was wrong. They reasoned to account for an evident and palpable truth upon false premises. Their candour would not permit them to withhold the fact, although they could not rightly account for it. They saw a truth in common occurrence, which they wanted to explain, but could not explain.

Others seeing that the fluid from Gonorrhœa did not produce Chancre, naturally concluded, because it possessed not the power of producing any local effect, no constitutional infection could be imparted from it. Even amongst those who admitted, for

instance, that the infection from both was the same, some of that opinion could not be brought to believe, that the constitution could ever be infected through Gonorrhœa, because they found it did not produce Chancre as the fluid passed out of the Urethra and lodged on the parts without the Urethra. For, say they, if this fluid possesses a power of infecting, prove to us that it locally does so, before we are requested to grant to you that it can, or does constitutionally infect? And for this reason, a Gonorrhœa has been pronounced by them to be harmless in its nature, to be alike to any other inflammatory attack, having no relation to virulence, for that it could not produce another venereal local symptom, and in the end cured itself. But fortunately for the truth of the question, some of that opinion to which I last alluded, have proved to their conviction by experiments, that gonorrhœal and chancrous fluids are from the same origin.

Where these experiments have been tried, the result of them has not been accordant with intention, but dependent upon accident; where gonorrhœal fluid has been taken from one subject and inserted into another, there a Chancre has been the consequence; but where gonorrhœal fluid has been taken and inserted into another part of the same subject, there no Chancre would or could have been produced by it—no more than it can or does produce a Chancre any where about the parts of the

Penis from the natural and continued flux of the gonorrhœal fluid from the Urethra.

Those who tried these experiments with the idea of ascertaining and confirming that Gonorrhœa and Chancre were produced from the same virus, were more at a loss than ever—were wider from the mark in accounting for the reciprocal innocency of these fluids, when found in their natural action upon the same subject—for their experiments brought them to confess, that the same fluid does indiscriminately produce both symptoms, and that the distinction between them consists only in the anatomical distinction of the parts on which the fluid is secreted. They of course knew this fact, but still the result of their experiments was at variance with the natural progress of both fluids upon the same subject. I have ever condemned this mode of ascertaining facts. An experiment made upon a principle in error, can only be an echo of that principle in error. It appears from this instance, that experiments in their result ought to be with caution listened to—for as these were originally founded in error, as they were injudiciously planned from the very beginning, whether they succeeded in proving the two fluids to be the same or not, that success depending upon the accidental and precarious circumstance of the experiment being made out of the fluid of the same subject, or that of another, they could not have ever been successful in fixing their fact, nor in proving why gonorrhœal fluid and chancrous fluid were

harmless to the same subject who secreted them. For the experiments which prove gonorrhœal and chancrous fluids to be the same, are not calculated to account for the innocent action of both upon the subject who secretes them. There cannot be the least doubt but they were induced to make these experiments solely because they saw that gonorrhœal fluid did not produce Chancre on the same subject; but in their attempt to explore the true cause, they offered a violence to Nature, and the common order of the case, by referring to another subject for the proof of that which was particular to the same subject, which other subject, in this instance, ought to have been totally out of the question.

If they had followed the dictates of the natural action of the poison, if they had watched the effects, if they had traced effects up to the cause, and if they had scrupulously informed themselves of those which in a common course of the natural operation of the poison were the most obvious and natural occurrences — this would have been the result of such an attentive and watchful observation.

They would have found from examining the woman who infected the man, after repeated proofs from repeated facts, that the two fluids were the same in their nature, and that the distinction between them was only a difference in the action of the same poison on different anatomical parts. — They would have also been convinced beyond a doubt, that gonorrhœal fluid on the same subject would

not produce a Chancre, and that no one Chancre would produce another Chancre on the same subject; but that if there be one or ten Chancres upon the same subject, and upon a variety of anatomical parts, if some appeared early and others late, every one of them were contracted and produced from the fluid which was imparted by the infecting subject. And further, they would have been convinced that a Gonorrhœa was never found to have been produced from the fluid of Chancre on the same subject. They would have seen a Chancre making its way upon the glans into the very aperture, eating away the commissures of the Urethra, and no Gonorrhœa ever produced in consequence.

By thus watching the natural action of the poison, and its natural and relative progress, they would have seen the extent of the power of it in the strongest light. By departing from this natural action, and this natural and relative progress of the poison, and by going in consequence into outrageous experiment, they turned themselves out of the only path which could have conducted them onward in the way to truth.

They have all along avoided meeting the whole of the question fairly, for those misgiving sceptics have been only seen to doubt one half of the question; they have all of them confined their doubts to Gonorrhœa being the same in origin as Chancre, that Gonorrhœa would ever produce a Chancre after any manner whatever, because no Chancre broke out in

consequence of Gonorrhœa upon the same subject. But they were never heard to say a syllable upon the other half of the question, which is much the strongest in the confirmation of my declaration, namely, THAT ONE CHANCRE ON THE SAME SUBJECT WILL NOT PRODUCE ANOTHER CHANCRE, NOR WILL IT PRODUCE A GONORRHŒA.

But now it remains for me to place the truth in a more conspicuous view, if it be necessary or possible, than I have yet hitherto done. I have shewn, and I trust the fact is satisfactorily established, that if the disease be imparted from one subject to another, from a subject infected to a sound subject, it is only necessary that the fluid from the infected subject should come into contact with any part of a sound subject, that by suffering it there to remain undisturbed, a possibility for infection will be given — and by this means it has the power of acting even upon sound skin. But I do not exact so much as this in my postulatam. I only desire the belief of common facts, that from a cut or sore upon the finger, practitioners in midwifery have been infected in consequence of their examining women who were pregnant, and who at the same time possessed one of the two symptoms, or both Gonorrhœa and Chancre. I trust that none of us are in want of further arguments to convince us, that the venereal poison may be propagated by inoculating a sound subject with the venereal fluid of a subject locally infected; and that it is from the want of opportunity

only, and not from the impossibility of the case, subjects are not thus more commonly infected. But in the observations which I am now about to offer, it will be apparent that the opportunity is always present, but that the ability never is.

If it were possible that our own venereal secretions could infect ourselves, how few young men or women could escape such action, when the gonorrhœal and chancrous fluids are discharged in such profusion for weeks or months, and when the patient is in the constant habit of fingering them? Where a pimple or a cut may absorb them? Where that which is received by the linen would infect, and where it would, to a moral certainty, if it were possible, how does it come to pass, that we find no instance of it upon record? Where the same fingers which handled these parts are applied to the lips, the nose, and the eyes? Where, from the slovenliness of some, and the poverty of others, the same handkerchief is often employed to receive the discharge, and to wipe the nose, the eyes, and the face? Where are the instances in consequence of this unwary indulgence of the experiment, if that any consequences of other infection, on other parts of the same subject, could be produced? I say for myself, I never saw infection fairly transferred after this manner; and I never read or heard of a case where it was clearly proved to be so. But if the infection could be transferred in this manner, who could escape infection on other parts through the

infecting fluid discharged for a continuance of time? These arguments are referable to the same unwary indulgence of the infection of not only gonorrhœal and chancrous fluids, but also of the fluid from a bubo. I have rubbed these fluids on the sores of the same subject over and over again, and never have been able to trace the smallest effect from them.

I shall reserve what I have more to say upon this question for the occasion which calls for it, and where it will also plainly tend to confirm and illustrate, in a constitutional sense, the natural action of the venereal poison.

ON CHANCER IN WOMAN.

Woman, as the counterpart of venereal connection with man, is exposed to similar venereal affections, but from the different structure of the parts, the same symptoms on woman will not be attended by such violent effects as they will be on man. The simplicity of the construction of their parts, and the parts being more exposed, together with the general temperance of a woman, prevent her being subject to those dangerous consequences which are often experienced by a man. No Chancre any where on the private parts of a woman can produce such effects as a malignant Chancre on the Penis of a man. But a woman is not exposed to a Chancre so frequently as a man, and it is not found so much in common with her as Gonorrhœa. There are two reasons for this — the one is, that the parts within the labia are se-

creting surfaces, and are more disposed to take on the symptoms of Gonorrhœa and to be inflamed, when the Vagina is infected ; and the other is, that the Penis deposits most commonly the infecting fluid beyond the parts susceptible of Chancre ; for when a woman has a Chancre, it is generally on the labia, although sometimes on the outer part of the nymphæ and clitoris. A Chancre may also be situated where the perinæum divides to form the labia.

The nature and relative importance of a Chancre in woman, as referring to constitutional affections, being exactly counterparts of the same complaints in man, I shall not dwell long upon this head. A Chancre neglected, or a Chancre acting upon habits predisposed to aggravate venereal symptoms, will be found to become desperate and malignant, not from any difference in the virus, but from a difference in the constitution on which it is acting. From the delicacy of the female nature, especially those of them who are infected, the mortification is most severely felt; symptoms of high inflammation will offer in practice, the Labia will require great attention, and a Chancre bordering on the Perinæum will, in consequence of the flow of urine upon it, be a difficult case for cure. Nothing is more common to be said, and nothing can be more founded in ignorance when it is said, than the observation which we often hear—that because a man finds he has a malignant set of Chancres, attended with all the aggravating circumstances possible, a woman who infected him

must necessarily also possess symptoms equally at least, if not more malignant. This is not necessarily so; a woman might, from a very small discharge, and when there is no inflammation, infect a man, who will be, in consequence, as severely attacked as it is possible for him to be. And this is just also in analogy with the Small-Pox — one may be inoculated from the fluid of another who died of it, and the one inoculated may have the disease in the mildest form. It is generally owing to the idiosyncrasy of the individual who possesses a disease, therefore, and not to the subject who imparted it, whether a disease be mild or malignant, rapid or slow, mortal or safe. And independent of this idiosyncrasy, it may also be owing to the bare condition of health; as from intemperate habits, sometimes the symptoms will be more severe than they would otherwise have been on the same subject at another season, when he had not committed intemperance.

What I have lately so much dwelt upon, namely, that our own venereal secretions are harmless to ourselves, holds as good, and is as strongly confirmed by referring to Gonorrhœa and Chancre in woman, as it does in man, and, if possible, it is more confirmed, for the discharge is more diffused, and more lodged about the parts: and the skin of a woman is much softer and thinner than that of a man. Now, if from one single introduction of a wounded finger of a Midwife, the disease can be imparted to another, or if one single venereal connection of a man, the

disease, as it is, can be imparted—how much more certain would the constant flux of venereal discharge, during a Gonorrhœa, produce a Chancre on the same individual, on any part where the fluid may be attached, if it were by the law of Nature possible to be effected, or a Chancre a Gonorrhœa *.

ASTRUC, the very indefatigable and very ingenious ASTRUC, has never lost sight of any circumstance which has occurred to him, either from personal observation, or from that of others. And ASTRUC says,—“ The disease is contracted by the introduction of the finger or hand into the place contaminated by a venereal ulcer or efflux ; by examining, for instance, venereal ulcers with a finger, or discharging the midwife’s office towards an infected woman, especially if the Surgeon’s finger, or Midwife’s hands have any wound or fissure, by which the poison may be more easily imbibed.”

So ANTHONY GALLUS relates, that he knew a midwife, who, by delivering a woman ill of the French Disease, was taken with it herself. So the Surgeon who translated CHARLES MUSITANUS into French, in his notes upon the fourth chapter of the second book, has produced two cases of the same kind, and so well attested, that both of them seem to be unquestionably true. So JAMES VARCELLINUS, in his

* Mr. John Hunter denied that a new-born infant infected with Lues Venerea was infected in Utero, and asserted that the infant so infected received the infection in its passage through the Vagina of the diseased mother.

Treatise de Pudendorum Morbis, et Lue Venereâ, declares, that he knew a youth, who being afraid of having to do with an infected woman, only wantonly griped the pudenda with one of his hands, and yet that hand swelled very much, and he afterwards broke out all over in pustules, so that he had liked to have suffered from a confirmed disease, for a very transitory enjoyment, if he had not called in the physical art to his assistance.

A Surgeon of eminence in this metropolis invited me to a sight of his patient, whom he is now attending for a venereal ulcer on his right fore-finger. The patient is a tooth-drawer; he had a sore upon his finger, which became venereally infected by drawing the tooth of a man who was constitutionally infected with the Venereal Disease. These instances have been, and are so common, that if I were to go about collecting the whole of them, they would be infinite.

CHAPTER VIII.

ON BUBO.

THE doctrine of lymphaticks being a system of absorbing vessels, every where dispersed upon the skin and the internal surfaces of parts, brings more assistance with it to lead us onward, not only to account for the progress of Lues Venerea into the constitution, but also for the progress of its antidote (mercury) into the constitution, and thereby affecting it, by the same channel, by the same method of conveyance. It is in reality by the absorption and progress of venereal and other poisons, that the doctrine of lymphaticks has been farther confirmed. The lacteals were discovered, traced, and their use ascertained, from the circumstance of a manifest and particular colour in their contents, upon some occasions at least. We had not the same advantage with respect to lymphaticks ; but in them, what we could not trace by the eye, we have found out by the effects of venereal poison. We know, from observation, that this poison may be absorbed on any part of the body, and thence diffused throughout the constitution. We must suppose it absorbed by the same vessels that absorb its antidote mercury,

or any thing else that is carried into the mass of blood by absorption, or as the bite of a viper, of a mad dog, of inoculation of the small-pox, or cow-pox. Many things, of a more inoffensive nature, pass unobserved; but this poison, from its inimical, irritating, and destructive quality, is disposed to raise a disturbance in its passage, before it reaches far enough to mix with the blood. Hence the lymphatic glands, through which every absorbed fluid must pass, are so often affected by venereal poison, when it is proceeding onward with its contagious effects into the constitution.

If infection be received in the common way, a Bubo is found on the groin, because the lymphaticks of the genital parts pass through the inguinal glands; but if the infection be received on the hand, a Bubo, for the like reason, is formed in the arm-pit; and when the disease is communicated by the lips, the glands of the neck inflame and tumefy.

In perusing Authors on *Lues Venerea* far back, we find their ideas of the theory of Venereal Bubo quite obsolete, from want of this knowledge. But the doctrine of lymphaticks was not unknown to ASTRUC, as it has been unfairly said to have been by a late modern Author: and although it may be possible that ASTRUC did not apply the doctrine to account for all circumstances which occur in this disease, but if he had so applied it, a more clear idea would have been formed by him; yet because he erred in some particulars, it must not therefore

be inferred, that he was ignorant of the system, and of the application of it altogether. Mr. HUNTER should have recollected, in common with us all, that we shall ever owe a tribute of praise to this wonderful Author, for the most laborious and most ingenious history of the origin of the disease, and of the symptoms, that ever was written; and so far from any modern Author going beyond him upon these points, on the contrary, without him, without our referring to him, the whole of his historical account would have been no where else found in one point of view, for he collected it from every where. We feel ourselves impelled by gratitude towards ASTRUC; and as Mr. HUNTER has chosen to produce quotations from ASTRUC, to prove he knew nothing of the lymphatick system, I shall not hesitate to declare, he does not merit such an implication of ignorance; and in proof I will produce a quotation from him, plainly shewing that he did know the true theory of a Bubo, that theory being well known in his time, being publicly taught at the foreign universities, of which this great man did not fail to avail himself.

“ Lastly,” says ASTRUC, “ the venercal poison is introduced by transmission through the lymphatic vessels, when the virulent particles being insinuated through the pores of the parts into the lymphatic vessels which are spread upon the skin, are carried, together with circulating lymph, into the nearest conglobate glands, and there exert their power.

Thus, tumours in the inguinal glands are apt to follow upon impure coition, Chancres in the private parts, or a slow-discharging Gonorrhœa ; so swellings in the maxillary and parotid glands succeed venereal apthæ, or ulcers of the gums, tongue, palate, or jaws, in such as have contracted the disease from sucking or kissing: so tumours of the axillary glands are apt to attend venereal chaps, fissures, and ulcers in the nipples, part of the poison being carried from the private parts to the inguinal glands, from the inside of the mouth to the parotid or maxillary, and from the nipples to the axillary glands, according to the laws of circulation, which the lymph observes." It is not possible for more truth to be expressed in fewer words: not only a perfect knowledge, as far as he has gone, of the system of lymphaticks appears to be familiar to him, but also the knowledge of its being a system of absorbing vessels ; for he gives to the lymph its own law of circulation, and he tells us also, and which is very true, that, from ulcers only locally contracted, the glands will swell, and not from ulcers which arise out of that constitution which is already infected ; but that the absorbed virus, from ulcers locally contracted, by passing to the first glands belonging to the absorbing vessels, will inflame and irritate such glands, and such only as stand in the way of its passage into the constitution. If ASTRUC has not availed himself of all the knowledge which this new system could have led him to, and has not applied the beneficial purposes

of it in accounting for many points which were but badly accounted for without it, if ASTRUC has erred on this side, if he has not pushed the new idea far enough, others since have erred on the contrary side, for they have, from the knowledge of the theory, applied it to a wrong purpose, have abused it, and laid the foundation of a wrong practice from that cause. This shall be hereafter explained.

Buboes are found on the inguinal glands from a venereal cause, which may be either Gonorrhœa or Chancre; and also when neither Gonorrhœa nor Chancre has previously appeared; when there is no trace, from the minutest examination of any one first infection to be found on the Penis, neither Chancre, Gonorrhœa, Warts, nor any other primordial symptoms; and in this instance a Bubo may be said to be a primordial symptom, when in the other instance it may be said to be only a secondary symptom. A Bubo is a symptom which proclaims that the virus has passed so far in part only (as ASTRUC observes) from the Chancre or Gonorrhœa with which it originated; for the virus has certainly in part only passed, or the Chancre would be disposed to heal; but so far from a Chancre healing, it will never, by any act, take on such a disposition until it be cured by mercury, but in that instance it will often heal, when its Bubo does not. A Chancre will become stationary, as the virus is absorbing, and it will remain so for some time when the groin becomes affected, neither changing for the better or

worse. The discharge from it, in that case, will be small, and as if it were unwillingly yielded. I have seen a small abrasion of the cuticle of the glans remain stationary until such time as two Buboës came to abscess, but from the effect of mercury the red spot on the glans has disappeared, and this was the only symptom which I could detect.

We are often told that a Bubo may be produced in the groin from other causes besides venereal; and we are also told, when the Lues Venerea is the first cause which produces it, that a Bubo may, notwithstanding, be not venereal. There are such doctrines. The foundation upon which one of these assertions is built, is from a knowledge that glands may be irritated by lymphaticks being irritated, and that glands may be also irritated by lymphaticks being wounded, and that this may come to pass, independent of any influence through either venereal or any other poison. The theory is undoubtedly true, and it is also true, that we very often see obstinate tumours in the maxillary glands of men, women, and children, from what is termed scrophula; that we see the glands in the groins tumefied and tender only from a wound in the foot, hip, or thigh; and that we also see the sub-maxillary glands swell and come to abscess, from a whitlow on a finger; but in all my practice, I have seldom seen the glands in the groins come to abscess without a venereal cause. I mean not to deny the possibility of the case, but I mean by this to exemplify the rarity of it. If these

possible cases, because they are barely possible, be constantly lugged into observation, whenever we are treating upon venereal Buboes, it will be like a truth told out of season, misguiding and confounding exceptions with general rules. If I saw two Buboes in the groins, and could not discover any other assignable cause for them, it would take me some time to be satisfied that they were not venereal, unless there were a moral impossibility against their being so.

It being a grave question to decide upon, whether a Bubo or Buboes be from a venereal cause or not, and it appearing that many more practitioners are found now a days to doubt very frequently, that a Bubo is venereal, which has been formerly decided in the same sort of instances to have been so; I should like, if I could, to draw up this question, and bring it into as small a compass as I possibly could. If a patient comes to me, having a Bubo in the groin, but without Gonorrhœa or Chancre, I should ask of him to tell me when he had a connection which was suspicious; I should examine if he had lately received any injury in the foot, leg, knee, or thigh; I should look also to his more general state of health, and how it was before he discovered this Bubo in the groin. If I saw him in a fair state, otherwise than from the affection of the Bubo, if I discovered nothing that could induce a Bubo in the foot, leg, knee, and thigh, nothing from his state of health, and if he says he lately has had suspicious

connections, and such as made him doubtful, I should be much disposed to pronounce such a Bubo to be a true Bubo, in consequence of venereal virus.

I have said, it has been also asserted, that a Bubo may not in the second instance be venereal, when in the first instance Gonorrhœa and Chancre were apparent. This idea was first propagated as an ingenious apology for that obstinate fact, which it seems to confirm, of the sameness in the nature of the poison which produces Chancre, and which produces Gonorrhœa; and also as an ingenious apology or escape for those who agree that the poison is the same, but who deny that the Gonorrhœa can produce a venereal Bubo, alike partaking of virulence as that produced by Chancre. Both these opinions being so contradictory to the common progress of the disease, and so irreconcilable to the common nature of it, that they who adopted them, observing the fact that Buboes did arise in consequence of both, were therefore obliged to assign another cause for the appearance of one sort of them, namely, that it was in consequence of sympathy, and that such a Bubo did not possess any venereal power of infecting the constitution.

A very ingenious young Surgeon consulted me for a Bubo which he had on each groin, in consequence of an infectious embrace. He was perfectly satisfied that such connection was the cause of them. These Buboes came to abscess, in spite of every attempt to discuss them; and these were the first

and the only symptoms which he had. Two very important points are illustrated by this case. The one is, that it must have been the infecting fluid of the woman which was absorbed, without leaving any local impression; and the other is, that sympathy could have had no other concern in the case, than having brought, in the first place, the lady and gentleman together.

Irritation does certainly, from many causes independent of venereal virus, tumefy glands in connection with parts thus irritated; and glands will also feel the force of this influence in common, from the pain only of Chancre, and from the pain only of Gonorrhœa. But there is nothing so very excessive in the pain of a Chancre, or in the pain of a Gonorrhœa, considering it abstractedly from all relation to virus, as to provoke such uncommon inflammation in the inguinal glands, as to bring them to abscess, and sometimes in consequence become the seat of the most obstinate, the most continued, and the most dangerous effects that can be adduced by the action of venereal poison.

Glands which are affected from evident common causes of irritation, as evidently subside when the first cause is done away: this is the consequence of irritation on glands from common obvious causes. But glands inflamed by the absorption of virus from a Chancre, grow more inflamed by the abatement of the original symptom which gave rise to the inflammation on them; at least the original Chancre does

not increase, as the inflammation increases on the glands, but it seems to be niggardly sparing that virus to the glands, which cannot be comprised within the limits of its own action. In order to clear my ground well as I go, I shall say that I take no farther notice of such Buboes in the groins, which disappear spontaneously, when the irritating cause which provoked them to swell abates; but that my object for consideration are Buboes, which from venereal virus remain sometimes hard and un-inflamed, or which come forward to abscess; such Buboes which owe no relative obedience to any influence of any common stimulus independent of virus, which will remain progressively advancing; and such Buboes which will become more and more formidable, which will to all moral certainty impart the infection to the whole of the constitution, and which cannot be weaned from the constitution by any general mode of treatment appropriated to glands diseased from any other cause—these are the Buboes which I am defining, and such only are produced by venereal infection.

It is always an object which will have its use in theory, and of course also in practice, to know how every local symptom of Lues Venerea will eventually appear, and how it will eventually terminate, when left to proceed, without any check or controul from mercury. And from what I have seen in these instances, it appears to me, that if the virus, by being left to its own uninterrupted action, proceed

through the lymphaticks, from a local infected part, onward into the constitution, the force of its effects will not operate so strongly on local parts, as it will when it is not with so much facility absorbed into the constitution.—For, in the first place, if the virus from a Chancre be absorbed, and the glands of the groin swell, that Chancre will not proceed with so much rapid malignancy as it had done before; and if the glands of the groin remain tumid and hard, yet uninflamed, it is most probable that the virus is passing from thence into the constitution; for whenever the constitution is discovered to be venereally infected, the glands of the groin are found to be more or less enlarged.

But as every subject who contracts Lues Venerea is not aware of the whole of the symptoms of it, as he only knows them from the whole of the consequences which the disease produces, as sometimes the venereal virus will pass from the Penis through the absorbents, without leaving any impresssion upon it, or if it does, it will be such an impresssion as will excite no attention from pain, or from such palpable appearances as will attract regard; so will the absorbents conduct the virus sometimes to the inguinal glands, from whence, with a very small sense of pain, it will find its way into the constitution, and shew itself in an unquestionable form, either by corroding ulcers in the tonsils, or blotches on the skin. This is known to come to pass, and when it does, if the surgeon refer to the glands of

the groin, he will find them more or less enlarged, although that circumstance was not before known to the patient.

I consider the lymphatic glands distributed upon the surface, in the light of barriers to the constitution, as sentinels which let into the constitution every thing that ought to pass by that channel for the good of the constitution, without the least restraint or difficulty, and without betraying any disgust whatever; but if any thing poisonous be offered to pass through that channel, and by which the constitution can be materially affected, if any thing attempt to pass through, which should not, because the constitution would in consequence suffer from it, an alarm and an annoyance are first expressed by the lymphatic glands, they become inflamed, and the sense of the injury which the constitution would receive, is imparted and forewarned by these glands, before the constitution ever receives the injury. But whether the cause of diseased glands be known or not, whether their disease be accounted for, either from the action of venereal poison, or from a hidden disposition in the constitution, it is found by experience, which the result of practice demonstrates beyond a doubt, that glands, when diseased, owing to their anatomical structure, are not so easily cured, or reasoned upon, as other parts of a different anatomical structure. There is nothing from analogy to that which is the best method of restoring other diseased parts of other anatomical descriptions, which

will assist us in the cure of diseased lymphatic glands. As they are proved to be the glands of a system of absorbent vessels, so have they their own peculiar diseases as well as peculiar functions, and so will they require a peculiar treatment for the cure of them.

I have one more remark to offer upon the subject of the lymphatic system, which is, that it is found to be more active in the young than the old—that when the subject is young, he is more exposed to Buboës than when he is old—and that when the subject is youthful, these glands are more conspicuous than as he grows old—and that as the skin shrinks, and becomes loose, the lymphaticks and the glands seem to have done their offices, and become in some degree obsolete.

I shall forbear to enter more particularly into every circumstance attendant upon hard and scirrhous Buboës, and more especially upon Buboës, which, when they come to abscess, assume the most dangerous appearances that offer in the whole history of Lues Venerea, until I come to consider the Cure of them.

CHAPTER IX.

THE ACTION OF LUES VENEREA ON THE
CONSTITUTION.

As the symptoms of Lues Venerea which are produced from a constitutional infection are various; as they are not so familiarly known to the patient who suffers them as local symptoms are; as they are often experienced at very remote periods; as the patient, during the progress of them, may be so situated, that from ignorance of the symptoms of the disease, the true nature of it might not have been discovered by him, or from the poverty of the patient, he might not have been able to avail himself of the advantages of professional assistance; so will the disease be often found to have been acting upon the constitution for a long time, and to have demonstrated all its effects without any controul by mercury, and without any disguise whatsoever in the process of it.

It has been already asserted, that it is by absorption of the original virus, the infection is conveyed to the constitution; that sometimes it is found to pass into the constitution, without making any impression upon that part which receives it from the subject which gave the infection; that sometimes it

is found to pass from the local impression of Gonorrhœa, or from the local impression of Chancre; and that sometimes it is found to pass from the local impression which it first makes on the inguinal glands.

As this is the natural progress of the disease in the first stage of it, an opportunity fairly offers for me to remark, that it is most probable the absorbents are commencing to convey the infecting fluid into the constitution at the very first period it comes within contact of them. But that on anatomical parts of a certain description, this process is more retarded than on others; that a Gonorrhœa in the Urethra is not so disposed to admit the absorption, as a Chancre behind the glans; that a Chancre behind the glans is not so much disposed to admit the absorption as a Chancre upon the skin of the Penis; and moreover, that when any of these local symptoms are apparent, the virus may not be absorbed from them alone, but it may be also absorbed where it has been lodged upon other parts, and on which parts it has not left any impression. The last observation which I made, and which goes to assert, that the virus received from another subject sometimes proceeds into the constitution, and there produces constitutional venereal symptoms, without having made any local impression, has never been denied by any Writers upon this disease, and therefore it stands upon record as a fact, which nobody dreams of disputing.

Whenever a patient applies for a case of confirmed Lues Venerea, the original symptom of Gonorrhœa or Chancre has most commonly disappeared, and had in some instances been totally forgotten; and in other instances, a patient has not been able to recall to his recollection the smallest trace of any primordial symptom by which the constitution has been thus infected.

It is not only always the foreign fluid received from another subject which infects the subject who receives it, but it is also the permanent action of this foreign fluid which supports the disease, and keeps alive its existence as long as it remains unextinguished. The fluid which is produced by the local action of the foreign fluid will be, to another, that which it was to the subject who gave it. It is therefore self-evident, that as our own secretions do not interfere, so that gonorrhœal fluid shall locally produce Chancre, or vice versa, no more will the progress of the foreign fluid, when absorbed, be either influenced in its action, be either advanced or retarded, be in any ways affected from the fluid. barely as a fluid which arises in consequence of a Chancre, Gonorrhœa, or Bubo, on the same subject. But all future venereal constitutional consequences depend totally and solely upon the actual absorption of that venereal fluid which was imparted from another subject; and that so far as it refers to the subject who possesses them, the symptoms of Gonorrhœa, Chancre, and Bubo, are only argu-

ments of the foreign virus being present, and acting upon these parts. But if the fluid of either be imparted to a third subject, it will act on that subject with the same power as the fluid which was received from a prior subject acted upon the subject who is infected by it, and who imparts it to a third subject.

C is infected by the impression which the venereal fluid has made on some part or other of *C* locally, or constitutionally. This venereal fluid was imparted to *C* from *B*. This fluid to *B* was a fluid produced in consequence of the venereal action of a foreign fluid which *B* received from *A*. The fluid secreted by *B* is the effect of the stimulus of the foreign fluid which *B* received from *A*; and to *B*, as a fluid secreted on a local part of *B*, through the action of the foreign stimulus received from *A*, that fluid will be harmless, will be merely the result of the action of the foreign stimulus communicated from *A* to *B*. But when *B* transfers this fluid to *C*, which was secreted by *B*, in consequence of the action of the foreign stimulus imparted by *A*, this fluid so transferred from *B* to *C*, will produce similar effects as a foreign stimulus acting on *C*, as that fluid which was transferred from *A* to *B*, produced on *B*; and so will the system of infection go on from *C* to *D*. The fluid which *B* secreted, must therefore be considered as a secretion proper to *B*, and therefore harmless to *B*; but as it was produced from a poison imparted by *A* to *B*, so does *B* transfer that poison, the same in its nature, to *C*, which *B* received from *A*.

The importance of this truth will solve most venereal phænomena, and lay them open to plain comprehension, with as much facility as it has, by the knowledge of it, explained already the sameness of the nature of Gonorrhœa and Chancre; and by this we know, they cannot interfere on the same subject. As the controversies which have been caused, from the appearance of these two local symptoms, failed in the true explanation of the matter of fact, so did every attempt which has been offered, in every essay which has been written upon the subject, fail not only of satisfying the judgment, but did also induce theorists to wander from the very path of truth, induce them to lose sight of the actual effects arising from the local action of the disease, induce them to deny a self-evident truth, that Gonorrhœa was a symptom of Lues Venerea, and induce them to found new controversies upon the errors arising out of a wrong definition, and upon a misunderstanding of the first fundamental points which were attempted to be explained, but not with success equal to the attempt.

Before I proceed to describe the symptoms of Lues Venerea, I shall lay down one general rule, which must be considered as an axiom, as a truth which pervades the whole of the theory I shall hereafter enlarge upon, and which must be considered as the only basis of it—That when the foreign virus which was imparted by another, is absorbed into the constitution, and when the constitution is con-

firmed in the infection, which I define it to be, when any outward visible signs of that confirmation appear, and which outward visible signs may be ulcers on the tonsils, blotches upon the skin, ophthalmia, ozæna of the nasal membrane, ulcers on the interior part of the œsophagus, gummata or deep-seated cavernous sloughs on the aponeurotic surfaces of the muscles, nodes upon the periosteum of any bone, caries of any bone itself, &c.—when one or all these symptoms do appear, or if it were possible to know what the disease was, whilst a previous symptomatic fever was on, and before the cause of that symptomatic fever was known to be venereal, by none of those venereal symptoms having yet appeared, I should not, even in that instance, hesitate to declare, that the constitution was completely possessed of the influence of the foreign virus, and that, in all such cases, any or the whole of the fluids taken from a subject infected, will impart the infection to a sound subject, provided it were so applied to that sound subject, as to give the fluid an opportunity of taking effect. It matters not whether the fluid appear to the eye in a sound wholesome condition; it does not depend, for infection to take place, whether that fluid which is expected to act effectually, be taken from an ulcer on the tonsils, or any other part of the constitutionally infected subject, and which has not the immediate signs of venereal infection; I do not assert, that the fluids from constitutional venereal ulcers, are the

only medium by which a subject, so conditioned, can impart the disease; but when the subject who imparts it, is notoriously infected with it, I do assert, that all the fluids, all the secretions are capable of imparting the disease to a sound subject, provided the full opportunity be given, that it may act as it can act, and provided it has the power of being left to do all that it can, and to produce what effects it can, by its action, when engrafted on another subject.

If the theory which I am about to confirm, by the self-approving natural occurrences in the history of venereal infection, wanted strength for its confirmation from analogy, I could support it by the effects of the bite of a mad dog*; and so far as the general law of the action of animal poisons is in accordance, it may, for that reason only, be justly appealed to. A mad dog does not become a mad dog because his saliva is possessed of the power of conveying canine infection, but the saliva becomes possessed of the power of conveying and imparting the infection, because the dog is mad; the dog being mad, his fluids possess the morbid influence; there is no more change in the appearance of the saliva, than in the appearance of any other fluid within the dog. A transplanted tooth, taken from the mouth of a subject constitutionally infected, and whose blood

* Mr. Nourse has published in the *Philosophical Transactions*, N^o. 445, “Where the poison in a boy bitten by a mad dog was latent near nineteen months.”—He adds, “which I know the books mention, but it never fell within my observation before.”

is venereally tainted, and selected, as I observed before, for its perfect beauty and soundness, on which, nor on the sockets of which, not the least trace of the disease can be seen, by such tooth being applied warm and moist to the socket of a sound subject, it will rapidly forward the most confirmed symptoms of Lues Venerea. Neither the saliva of the mad dog, nor the tooth of the diseased subject, can be said to be apparently under any change from the disease of either, although in reality both are in full possession of all the poisonous influence which they are found to impart.

These outlines of the action of venereal poison, which I have given, intirely correspond with not only the natural progress of the venereal poison, but they are warranted and supported by every Author who has written on the disease, unto the time of Mr. Hunter's publication, who took upon himself to oppose them. Whenever I read an Author upon Lues Venerea, I try to find out what his object may be in writing, and I give him my credit, or not, accordingly. When I read Astruc, I do not expect from him an improved method of cure, nor do I read for that purpose—I expect from him no more than a true plain narrative of the action of venereal virus, how it is imparted from one subject to another, and what is the result of its action. I expect to find in his book a zealous account, founded upon the most incontestible arguments, of the whole of the natural progress of venereal infection, and of the

whole of the forms in which it may be communicated. As he devoted his attention to this particular object—as his publication, when it came forth, was open to the eyes of all Practitioners—as he had the full opportunity of availing himself, both as a man of letters, and a man of the highest estimation, not only of that knowledge which Authors of every country could furnish him with, not only of that which the result of his own inquiries illustrated, but also of the cases of other Practitioners who lived at his time, and who partook of his friendly conversation, upon his favourite topic, at the time he was forming his Treatise on Lues Venerea—so does he deserve full credit for what he has said on the action of the poison.

ASTRUC has attributed to the Lues Venerea as broad an outline, as great extent of power, and of natural action, as I have. ASTRUC has laid down every form by which the disease can be communicated: from the incontestible proofs of cases which came within his own knowledge, and from the natural accordance of venereal action, he established his theory upon the truth of observation. These new observations which I have subjoined, I am able to declare, tend to confirm the assertions of ASTRUC.

When the venereal poison has taken full possession of the constitution, not only the parts which indicate the disease to be venereal, by their apparent morbid vitiations, but also every part of the constitution is venereally vitiated. This vitiation of

the whole of the constitution is not brought on from any absorption that may take place of that discharge which is produced by the local action of the foreign virus, but it is the absolute action of the foreign virus alone, which, by being absorbed, brings it on. The local action of it, therefore, is no sine qua non of infection. And in the next place, the absorption of our own discharge, being an absorption of a harmless fluid, it can neither act for or against the constitutional infection. I shall not hesitate to assert, that the venereal infection, from its nature, is not so limited as Mr. HUNTER declares it to be ; and I shall prove, that from want of knowledge of this leading first principle, namely—that our own secretions cannot infect ourselves—many of his experiments which tend to limit the natural action of the disease, and to narrow the power of it, and that in the very face of cases to the contrary, were founded in error, and could not, from that cause, demonstrate either the force of the disease, or its true natural action, either on the subject who possessed it, or on one to whom it may be imparted.

I shall now offer that which I understand to be the power of Lues Venerea, both as to the subject who possesses it, and to the subject to whom it may be imparted. And in these leading points, I shall be found to differ from Mr. HUNTER. In all these differences where I do not prove him to be wrong, there I wish him to be believed to be right ; for I am impelled from no other motive, than that of the

exalted esteem I have for the truth and honour of our profession. Foreigners have condemned his theory, and perhaps they may think it contains a catechism of the general opinion which we, in this country, entertain of this disease*.

Whatever is possible, may not always come to pass. No one ever expects that possibilities should be, in every instance, realized. If a disease has the power, that is all which is exacted by a possibility; and that is the sense which I conceive of what may be possible. I therefore say, that all and every part of the constitution is vitiated, when the avowed symptoms of Lues Venerea are apparent, and when they are not apparent, from having been obscured through mercury. And I do in consequence say, it is possible that a subject thus infected, will get a child in whom the venereal rudiments will grow, with every other natural quality which is imparted

* (Translation). GOTTINGEN 1787, Vol. III. P. 1922.

"J. HUNTER's Dissertation on Lues Venerea from the English, with three Plates, 1787, pp. 688, in great 8vo. exclusive of the Register.—

"There are not so many annotations and most necessary informations, as in page 339 of the knowing and learned translator, as would have been necessary for making this book, by a translation, only universally useful; which, in his original, contains so many singularities and numberless errors. It is to be wished that the translator had (as in page 140) used throughout the whole book the admonitions which J. FOOT has made against it. In this case the translation would have had a very great preference of the original itself."

to the child from its father. Also, that the mother of a child venereally infected, will produce a child venereally infected; and that these occurrences are very common to those who seek for them in this populous place. But all Authors have asserted such to be the natural power and extent of Lues Venerea, excepting Mr. HUNTER; and our countryman, WISMAN, has given us instances of it in his folio edition, page 491.—And so has ASTRUC.—But more will be said upon this question in another part.

I shall also assert, that chaps in the lips may be the vehicle of infection, by kisses received from a subject who is constitutionally vitiated—that the repeated venereal intercourse of a sound person with a person whose constitution is venereally infected, will impart the disease—and that the disease is often produced this way, when, upon examination, nothing is seen about the private parts of a woman to warrant a cause of infection; when at the same time she may have, on other parts, strong proofs of constitutional infection. The following is a case in point.

A gentleman applied to me, with ulcers in his throat, and which had been there for some time. His constitution seemed to be very much deranged, his rest was disturbed, his pulse was quick, his skin was parched, his appetite failed, his spirits were sunk, he was grown thin, and his mind was indolent. Upon looking at the tonsils, I began to question him, whether he had any indicative symptoms

any where else? and found his prepuce thickened and horny, it could not be slipped back, a profuse nauseous discharge was yielded of the sebaceous secretion from withinside of the prepuce, and the inguinal glands were distinct, enlarged, and knotty. I then made known to him the certainty of his case being venereal. He then told me, that for more than nine months, he had only connection with the same woman, and who also had at this, and for a long time before, a sore throat, and some spots on her skin; but that since he had taken her to himself, she had neither a *Gonorrhœa* nor Chancre. I examined the woman, upon whom I found no other symptoms but those already related: but there was an induration on one labium, where a Chancre had formerly been, and the glands of the groins were enlarged. The fact also was confessed, that she had been taking mercury at times, so as to baulk the progress of the disease, but not to extinguish it. It was the absorption of her vitiated secretions which infected the gentleman. Both submitted to a regular process of cure, and the symptoms of both were completely eliminated.

A nurse infected, may impart it to a sound child who sucks her; and an infected child may impart it to a sound nurse, by sucking her. Venereal vitiations, from these causes, constantly offer. And those who approve of Mr. HUNTER as a theorist, will not so far pin their faith upon him, as to become indifferent about the state of a nurse who is to suckle their child.

*“ The opinion of a man so justly celebrated as JOHN HUNTER is of great importance: we will examine it with the greatest attention, whether here or elsewhere. In his treatise on the Lues Venerea, which is well known in France since the year 1787, by the translation of ANDIBERTI, JOHN HUNTER speaks, in the first place, of Lues Venerea in newborn infants; in examining the different ways that this disease may be communicated. He fears, he says, that this disease manifests itself upon other parts of the body besides the genital organs; such as the anus, the mouth, the nose, the eyes, the nipples of the nurse that suckles them, of which the children have been infected in their birth, the genital parts of the mother being at that time infected†.

It is supposed, says he, in another part of his work, that a *foetus* in the womb of its mother who is infected, may receive the disease, and be infected as if there was a direct communication between the two. I doubt much that it can happen, from what I have already said, speaking of secretions; for you see that the same matter that will produce a constitutional inflammation, is not able, as I have said above, of communicating the disease. You may nevertheless conceive how possible it is for a child to be infected in the womb of its mother who is in-

* Extract from a work published by the late P. A. O. Mahon, chief Physician to the Venereal Hospital du Vaugirard, &c. &c. at Paris.

† *Malad Vénér. de Jean Hunter*, chap. 2. p. 25.

fectcd, not by the disease of the mother, but by a part of the same matter that infects the mother herself, and which she has absorbed ; and whether this matter is determined or not to the solids of the mother by its action, it is possible that it can pass to the child as pure as it was absorbed, and in this case it may infect the child precisely in the same manner as the mother is infected*.

“ We find, in the same chapter, cases which prove this communication. A woman aged twenty-five years was brought to St. George’s Hospital the 21st of August 1782, with ulcers on the legs and pustules upon the body. Her husband was a soldier, and had given her the *Lues Venerea* in December 1781. The symptoms that she had was a discharge from the vagina, with a little swelling of the glands of the groin, which were painful. She had taken thirty pills that were supposed to be mercurial. In February 1782, about three months after she was infected, the discharge stopped ; but the swelling, which encreased gradually from the instant of its appearance, had at last suppurated. She applied an ointment which they gave her, and two months afterwards, that is to say in April 1782, she was cured. After the cure of the *Bubo*, the discharge reappeared : she took a second time a larger number of the same pills, of which she had taken before to the amount of thirty ; but immediately afterwards

* *Malad Vénér. de Jean Hunter, chap. 31.*

her body was entirely covered with pustules, of which some that were upon the breast, upon the legs, and upon the nipples, ulcerated. The twins which she had at eight months, in May 1782 (the time that the Bubo was approaching to its cure) were covered with pustules at their birth, and died very soon after. Another child, aged about two years, which she suckled, was also covered with pustules when she came to the hospital. The twenty-first of October, 1782, we placed the mother and the child in the room which was appropriated for those who were under salivation. The child took no mercury; they thought its gums were affected and a little ulcerated and the pustules were cured.

“What I have extracted from the works of J. HUNTER, proves, that he admits and knew Lues Venerea in new-born infants, but that he limited the mode of transmission to the inoculation of the virus at the time of its passage through the Vagina. He has, in another part of the work, thrown doubts upon the communication of Lues Venerea in new-born infants. But I shall reserve the examination and the discussion of these doubts to the time with which I shall occupy myself upon the objections made upon the Venereal infection of new-born infants and the mode of its transmission from the mother to the child, and the child to the nurse, and from the nurse to the child.”

So much does the venereal vitiation pervade the whole of the constitution which possesses it, that

even the blood of one infected being engrafted upon a wound of another, which other was previously a sound subject, will most completely impart the disease, and will impart it with more rapidity, than if it were introduced by the usual mode of infection, namely, through the absorbents. If the cases, which are from every where collected, be not forgeries, then must it be admitted, that every fluid of the constitution of one venereally infected, does virtually and actually possess a venereal vitiation ; and that venereal vitiation, although not in the least apparent to the eye, is capable of imparting the infection to another, when properly applied for that purpose ; even, I say, if there be no local venereal affection upon the part apparently from whence the fluid be taken, and which does convey the infection.

In the days of Astruc, modern refinement had not gone so far as to tempt beauty to purchase, for the sake of ornament, a tooth out of the mouth of a poor girl to be placed in her own, and to supply the place of one more ugly, or one that was decaying. That refinement was reserved for our time : and as good, in the common round of human events, is often found to arise out of evil, so has this new practice, from the unsuspected evil consequences which have been found sometimes to attend it, furnished us with a stronger insight into the hidden and occult nature of the Venereal Disease, and the power of its action, than all the cases that have ever before occurred.

I shall relate a case, which has already made some noise in the world, and produced an alarm in the minds of those who are interested in safety or in danger from transplanted teeth. This remarkable case has already appeared three times in print, by the late Sir William Watson, by Mr. Hunter, and by myself, in my third part of *Observations upon the new Opinions of the latter*. There I have been more explanatory upon the case; for that reason I shall only now barely state it, and refer for the comment.

A lady aged twenty-one, was attended, at the day appointed, for transplanting an incisor tooth on the upper jaw, at the desire of a dentist, by Mr. Hunter. He examined the girl from whom the tooth was to be taken, inspected the tooth when drawn, and delivered the tooth to the dentist for insertion. A little uneasy sensation continued for two days; at the expiration of that time, the sensation was found to increase, and to such a degree, that the lady applied to the dentist for relief. The dentist having examined the tooth, conceived it to be a cold caught after the operation, and recommended treating it accordingly, saying that he could no otherwise account for it. The lady then went into the country, the pain constantly existing and increasing; she was from thence obliged to send to town for the dentist. The dentist, after examining her mouth, said, he had still hopes of its eventually ending well; but begged, in case of its growing worse, that she would

come to town, and be in the way of constant attendance. This was accordingly complied with, and she remained at the house of the dentist for ten days. During this time Mr. Hunter saw her, and found the edges of the gum beginning to ulcerate, and to which he applied lint, dipped in a strong solution of corrosive sublimate. At the end of ten days the lady removed from the house of the dentist to Knightsbridge, and the late Mr. Pott was sent for to her, who applied something to the ulceration, gave her bark in decoction and substance, and also opium. The tooth which from the beginning had never fastened, was now out of her mouth. There was a consultation between Mr. Pott and Sir William Watson. The ulceration continued to spread under the use of bark; it had extended along the palate towards the uvula, and the maxilla superior became a caries. She had a symptomatic fever, and eruptions followed it; some of these formed into ulcers, and those on the scapula and the knees were corroding and cavernous. Nodes appeared then upon the bones, on the radii, and also on the tibiæ. She had exquisite nocturnal pains. She went through a complete course of mercury; it was administered in the form of pills and of unction, and the whole of the symptoms yielded to the action of mercury. She was removed into Hampshire, the place of her nativity, and died shortly afterwards in a hectic state. Mr. Pott never hesitated to declare, that if ever there had been a venereal case more

strongly marked by true venereal symptoms than any other—this was that case.

But I must observe, that the proof of the virus being thus conveyed, is not confined to this case. There are other cases gone forth into the world, from other parties and other channels, all of which yielded to mercury, and upon which there was but one opinion also, namely, that they were truly venereal.

These cases shew what the virus can do, when they do happen, but an opportunity does not often serve for us to ascertain what the virus can do from extraordinary cases. We all know what the virus can effect, when we see a Chancre upon the Penis, because this is the ordinary mode of communicating the disease.

There are two medical gentlemen within the sphere of my acquaintance, who have been infected by contracting Chancres on their fingers. These cases are within my own knowledge. One of them had bitten off the cuticle which hung loose on his right fore-finger, and which generated into a Chancre, from handling an opened body after death ; so that he was contaminated, not by ulcerous fluid, nor chancrous fluid, but by a general venereal vitiation of the fluids. This Chancre on his finger did not nor would not heal, and within three months eruptions broke out over his body, and which, with the Chancre on his finger, gave way to mercury. The case of the other proceeded farther, so as to affect

the periosteum of cylindrical bones; and as this gentleman dreaded a decided mercurial course, and depended on the alterative, he was more than two years in getting well.

It is not enough for a man to say, that he does not believe this, or that—this is no answer to proofs and arguments, no answer to strong cases and to strong opinions. It is fitting that I apply here what has been said by Tully upon a like occasion—that there is nothing more unbecoming, not to say a Physician or Surgeon, but even any person, than to object to his adversary, which, in case it be denied, he can bring nothing to prove it.

I shall now proceed to corroborate some truths of the action of venereal virus upon the same infected subject, to consider its action as relative to the possessor, and by applying my theory and my observations to this object, to see if I cannot explain that which appeared to be impossible before. Whatever is true, will always be easily understood; and whatever is false, cannot be but with difficulty received, and reluctantly acknowledged for a truth.

Mr. Hunter says, “When the matter is got into the constitution, it from thence produces many local effects on different parts of the body, which are in general a kind of inflammation, or at least an increased action, occasioning a suppuration of its own kind; it is supposed that the matter produced in consequence of these inflammations, similar to the matter from a Gonorrhœa or Chancre, is also vene-

real and poisonous. This, I believe, till now has never been denied; and upon the first view of the subject, one would be inclined to suppose, that it really should be venereal: for first, the venereal matter is the cause, and again the same treatment cures both diseases; thus mercury cures both a Chancre and a Lues Venerea. However, this is no decisive proof, as mercury cures many diseases besides the venereal. On the other hand, there are many strong reasons for believing that the matter is not venereal. There is one curious fact, which shews it is either not venereal, or, if it be, that it is not capable of acting in some respects on the same body, or same state of constitution as that matter does which is produced from a Chancre or Gonorrhœa. The pus from these latter, when absorbed, generally produces a Bubo, as has been described; but we never find a Bubo from the absorption of matter from a pocky sore; for instance, when there is a venereal ulcer in the throat, we have no Buboes in the glands of the neck; when there are venereal sores on the arms, or even suppurating nodes on the ulna, there are no swellings of the glands of the arm-pit; although such will take place if fresh venereal matter is applied to a common sore on the arm, hand, or fingers. No swelling takes place in the glands of the groin from either nodes or blotches on the legs and thighs. It may be supposed, that there is no absorption going on from such sores; but I think we have no grounds for such supposition. Its

mode of irritation, or the action of the parts affected, is very different from what happens in the Chancre, Gonorrhœa, or Bubo, being hardly attended with inflammation, which in them is generally violent. It might be supposed, that a constitution truly and universally pocky, is not to be affected locally by the same species of matter; but from the following experiments it would appear, that matter from a Gonorrhœa or Chancre is capable of affecting a man locally that is already poxed; and that matter from pocky sores, arising from the constitution, has not that power. A man had been affected with Lues Venerea a long time, and had been several times salivated, but the disease still broke out anew. He was taken into St. George's Hospital, affected with a number of pocky sores; and before I put him under a mercurial course, I made the following experiments: I took some matter from one of the sores upon the point of a lancet, and made three small wounds upon the back, where the skin was smooth and sound, deep enough to draw blood. I made a wound similar to the other three, with a clean lancet, the four wounds making a quadrangle; but all the wounds healed up, and none of them ever appeared after. This experiment I have repeated more than once, and with the same result; it shews that a pocky person cannot be affected locally with the matter proceeding from the sores produced by the Lues Venerea. But to see how far real venereal matter was capable of producing Chancres on a

pocky person, I made the following experiment: A man who had venereal blotches on many parts of his skin, was inoculated in sound parts with matter from a Chancre, and also with matter from his own sores. The wounds impregnated with the matter from the Chancres became Chancres; but the others healed up. Here then was a venereal constitution capable of being affected locally with fresh venereal matter. This experiment I have likewise repeated more than once, and always with the same effect*."

By this quotation which I have produced from Mr. Hunter, it appears, that from want of knowing that important discovery which I have preferred for consideration, all his observations were vague and indefinite; that he was making experiments in expectation of coming at a truth, but could not, through them, find it out. Had he already discovered the truth, these experiments would have tended to confirm it. Had his opinions upon the nature of venereal virus, as it affects its possessor, been sound, these experiments would have illustrated them. But so difficult was it found by him to hit upon the truth, that nevertheless it was by these experiments tempting his understanding, yet he could not embrace it.

Mr. Hunter says that, in consequence of these experiments, and in consequence of observation, "there are many strong reasons for believing, that the matter from venereal constitutional ulcers is not vene-

* Pages 292, 293, 294.

real." If this were true, there would be a strange falling off indeed of that extent of power in virus which I have attributed to it. For if I say that the cases of the teeth were venereal, and that the cases were produced from a venereal vitiation in the fluids of the subjects who imparted the infection; and if he says that even pus from venereal ulcers is not venereal, and will not impart the infection—one or other of us is very wide indeed of the mark. Mr. Hunter says, that "the pus from Gonorrhœa or Chancre, when absorbed, generally produces a Bubo, as has been described; but we never find a Bubo from the absorption of matter from a pocky sore," &c. I say that it is not the absorption of fluid secreted from our own Gonorrhœa or Chancre, which produces a subsequent Bubo, and that it could not possibly happen from the absorption of our own secreted fluid; but that it is always produced from absorption of the foreign fluid. Mr. Hunter was aware, if Buboes followed ulcers which were in consequence of venereal constitutional infection, that the lymphatics must first be affected through these ulcers, and that the fluid of these ulcers must be absorbed. He, not knowing at the same time that it was never the absorption of our own secretions which did inflame the lymphatic glands, took it for granted that the venereal fluid from a constitutional ulcer possessed no venereal power of infecting another subject, because it did not, through absorption, stimulate any gland which

was in the way of being stimulated, if it really possessed any such power. Mr. Hunter, not being in possession of this fact, took it for granted that such ulcerous fluid was innocent, because it was innocent to the subject who possessed it, and who secreted that very fluid which Mr. Hunter applied in making his experiment in order to ascertain, not the relative innocence of this fluid upon the subject who secreted it, but the innocence of the fluid, by its not propagating venereal infection to another, upon whom it might be applied. That is, in other words, Mr. Hunter wanted to clear up a point which he was not decided upon — whether venereal fluid, from a constitutional ulcer, was or was not equally venereal as the fluids from Gonorrhœa and Chancre. And very fatal for the truth of the question which he was seeking to ascertain, he just did that by his experiments, which could not ascertain the point he was aiming at. For he applied the ulcerous fluid of the same subject upon a part where no ulcer was, and found that it would not re-infect; and applied the chancrous fluid of another subject, and found that it would infect. Now, if he had applied the ulcerous fluid (by ulcerous fluid I mean to express the fluid from an ulcer arising out of a constitutional infection; and by chancrous fluid, I mean to express the fluid of an ulcer or Chancre from a local infection) upon another subject instead of the same subject, he would, by that means, have found what he sought for, namely, that it would have conveyed

infection : and if he had applied chancrous fluid to any part of the same subject, then he would have found that the chancrous fluid, no more than the ulcerous fluid did or would impart any infection, would in the least affect a part locally on the same constitution, no more than if the operation had been made with a clean lancet. But in these two experiments, the ulcerous fluid of the same subject was applied to ascertain its malignancy, and the chancrous fluid of another subject was applied to ascertain its malignancy also. Can there be any longer a wonder that the former should fail of taking effect, and that the latter should succeed ?

But I will now point out how he should have made these experiments in order to acquire new knowledge, and in order to acquire true knowledge. In the first place, his experiment with chancrous fluid was no more an object of his choice, than his experiment with ulcerous fluid was ; for he was, by not dreaming of this distinction which I have pointed out, seduced into an error. The subject upon whom he pitched to make the first and second experiment, being possessed of ulcerous fluid, he thought, very unsuspectingly, that there was no difference, as it was ulcerous fluid, whether it were tried upon the same or another subject ; and, therefore, when he tried it upon the same, he took it for granted that it was all the same thing, that it would produce exactly the same effect as if it were tried upon another sound subject.* In a moment of triumph, he seized upon

the opportunity of ulcerous fluid being at hand, for ascertaining this fact, and by it, for ascertaining a general theory, and a general effect of that venereal action of virus by which it imparts the disease from one subject to another. For he means to say, if this ulcerous fluid taken from a subject, and properly placed on a sound part of his skin, does not there produce Chancre, why then the ulcerous fluid does not partake of venereal infection, and it is not like chancrous fluid, for chancrous fluid, thus applied, does convey venereal infection. Therefore, says he, chancrous fluid is venereal, but ulcerous fluid is not venereal. But how was a part of his experiment proved? I mean that part of his experiment which refers to Chancre.—Why, thus it was proved—The subject, having only ulcerous fluid, he made use of his own ulcerous fluid, but he borrowed the chancrous fluid from another subject. For he says, that “he inoculated the man from the chancrous matter, and took also the matter from his own sores; and that the wounds impregnated with the matter from Chancres, became Chancres, but the others healed up.” I do not see, by making this experiment with chancrous fluid taken from one subject and applied to another, much more insight could be given into the nature of venereal action, than by the common mode of infection, for in reality it is no more. And if this experiment had failed, and it most certainly would have failed, had the circumstance of the experiment or the convenience of it suited the present

state which I am about to give of it—had that been put in practice, it would have most assuredly failed : and then I suppose we should have been told, and we might have been so told with exactly as much truth, that chancrous fluid would neither infect. For if the subject on which he made these experiments had had local venereal Chancres upon himself, instead of venereal constitutional ulcers ; if he had exactly reversed what he did, if he had inoculated the subject on another part with his own chancrous fluid, and had also inoculated him with the ulcerous fluid of another, then he would have found that his own chancrous fluid would have left no impression, but that the ulcerous fluid would have in consequence produced a Chancre, because this ulcerous fluid was the production of another, and from a constitution which was venereally infected.

In short, had he founded his experiments upon true theory, instead of founding his theory upon his experiments, he would not have misled himself, nor would he have misled others ; for I do not believe, that in this instance, Mr. HUNTER meant to mislead, for if I thought so, I should not hesitate to declare it. But this will shew the vanity of that theory which is formed out of experiment—experiment will confirm true theory, but no true theory can be obtained by experiment, for experiments may thus again be wrongly made. The circumstance of not being awake to, and of not having the mind open to receive an impression of that natural distinction

which there is in a relative action of one's own fluid upon ourselves, and in its general action, when imparted to another, will make all the difference in a true or false theory, in a true or false idea of poisons, in a true or false opinion which may be given of common venereal occurrences, in the danger or safety of a patient; and moreover than that, in ascertaining whether we know any thing about the matter or not. Another lesson is inculcated by this, that we should not be so sufficient in ourselves, as not to think any thing of the sufficiency of all the rest of mankind.

The natural distinction which ought to have been observed between fluid, as it relates to the subject which secretes it, and to the subject to whom it is transferred, is a key-stone to all the theory which can be in truth received for the confirmation of the nature of this poison, and of all others. Every ancient Author sufficiently impressed upon our minds, by the strongest cases, that which the venereal virus was capable of effecting. The venereal virus was always uniformly the same. Authors knew before us, and we not only were taught by them, but daily practice confirmed to us, that such being the subtilty of the disease, it appeared at very remote periods, after a probable opportunity were given for contracting it—that children were infected with it, we knew not how—that husbands and wives were attacked with it, they knew not how—that chastity was in danger of being calumniated, from the im-

perviable subtilty of the poison—and that from many and established histories of unaccountable cases which have occurred, suspicion was always awake to fear and dread of its subtilty.

I have said, histories of the natural progress of the disease convince us of these truths. But notwithstanding these were known to come to pass, yet from want of this distinction, of this key-stone, of this knowledge of relative action in the poison, neither our ancestors nor our cotemporaries have been able to form that theory, which exactly corresponds with and demonstrates its truth by proof, and which was known to exist, but not how it came to pass. It was to prove these assertions of our ancestors; it was to ascertain what was a possible power in virus, it was to elucidate that which occurred in the history of the disease, that induced Mr. HUNTER to appeal to experiments. Mr. HUNTER with truth saw that these occurrences, which were said always to have happened, and which certainly can and will happen again in the history of cases, were never accounted for: he therefore appealed to experiments, in order to account for them. He did that which the best and first of all mankind can only do.—None of us can reason, but from what we know; and through that limitation of human power, Mr. HUNTER failed. He made those experiments wrong, which were to confirm a circumstance already known to be true. This is the point at issue. These extraordinary circumstances of the subtilty of the

poison were known and confirmed, in instances indisputable. Experiments were only wanted to explain them. They were not wanted to prove whether they had or could occur. Nobody that knew any thing of the history of the disease, nobody that had read, nobody that had practised, ever doubted this fact, but nobody could or had explained it.

Having said thus much for Mr. HUNTER, I will now point out where he was to blame. He was to blame in affecting to despise an acknowledged power in venereal action. He was to blame in presuming to suppose, that his experiments were right, and that histories of the subtilty of the poison, in all the great variety of cases, were false. He was to blame in concluding that the nature of venereal poison must bend to his experiments. And he was to blame in not paying more regard to what had been well attested of the power of venereal action ; in not seeking his instructions from the history of cases, and from the most common symptoms, as they most ordinarily occur. Had he been faithful in observing the natural action of the disease, had he been true to Nature, knowing that she is always constant, that she never deceives, and had he only attended to natural events, and reasoned upon them, he would have been right.

If he had reasoned thus, he would have been right.

If I take fluid which is secreted by a subject, and insert it upon any other part of his skin, will that effect more than it does by always being liable

to be absorbed from a part upon which it is constantly dwelling? Can I expect, that by removing the fluid from one part to another, it will any more act from that cause, than it has been known to act, when opportunities for its action have constantly offered? And yet in all the various cases of long and protracted Gonorrhœa, have I ever seen a single instance where it has so acted? From the same reasoning, why should I take chancrous fluid, which dwells upon the Penis, and which neither produces another Chancre, nor Gonorrhœa on the same subject? What am I to expect from such an experiment, if I did transfer this fluid from one part of the same subject to another? Can I expect to confirm the true nature of the disease by it, if it should, thus tranferred, excite another Chancre? Does not the true nature of the disease revolt at the idea? Did I ever see that the fluid does generate fresh Chancres? Or if it did, should I not then see constant succession of Chancres on the same parts, and would not the whole of the glans and parts contiguous thereby become a galaxy of Chancres? What can I expect in consequence of my transplanting ulcerous fluid from one part to another of the same subject, but disappointment—seeing that parts surrounding ulcers are constantly exposed to the action of their fluid, if it had any—seeing that lymphatic glands would become inflamed, if absorption were virulent—and seeing that another Chancre was never generated from it?

Such must have been the result of his observing the natural action of the virus. Such is the natural effect of the fluid upon the individual who secretes it. Not only the disease owes its support, but it cannot be conveyed from one subject to another, without a transfer of the fluid, in the most ordinary way of infection, being absolutely made over from him who secretes it, to him who is infected through it. Such is the fact, that the whole of the secretions, as it is exemplified in the case of the tooth, which, to all appearance, are not affected locally by the disease,—that even the natural secretions of one constitutionally infected, do, when engrafted upon another subject, in extraordinary cases, produce such symptoms as all those who have seen them, know them, and positively define them to be venereal.

In making our observations for the discovery of truth, we should be conducted implicitly by Nature; she only can point out the right path, which we are in search after; we are blind, she alone possesses the sight; we are ignorant, she alone can instruct. We therefore should always follow her, and not attempt to pursue our way without her conducting hand.

I have now established the extent of the power of venereal virus, as it influences the individual who secretes it; and I have also established the extent of power of venereal virus, when it is imparted to another: here I mean to rest the question.

CHAPTER X.

ULCERS ON THE TONSILS, AND ERUPTIONS
ON THE SKIN.

FROM what I have already said, I have no occasion to go minutely into questions which may hereafter arise, and which bear relation to the theory of our own secretions having the least influence in supporting the disease, or of having any concern with the action of it on the same constitution. I am convinced by unerring observation, that Lues Venerea is not supported or aggravated on the same subject by any of that fluid, which is either produced in consequence of local infection, nor by any of that fluid which is produced in consequence of ulcers on the tonsils, of ulcers on the skin, or of ulcers situated on any other part of the body whatsoever. This position is not only just, not only proved to be true, in consequence of our researches into the effect of this fluid, but it is also in accordance with the general nature and action of all poisons.

Supposing we pursued the question, in order to try by it what would be the consequence to the same individual, if his own fluid, from any venereal ulcers, had any power or concern in supporting or renovating the disease, such fluid must be either re-absorbed into the habit, and by passing through

the lymphatic glands, stimulate them, or it must generate fresh ulcers wherever it were lodged so as to have an opportunity of generating them. Or, if the fluid, for example, which is discharged from venereal ulcers on the tonsils, possessed a power of acting malignantly upon a constitution which secretes it, what would be the consequences locally and constitutionally, which we may have a right to expect, from that being a fact? (They would be nothing less than that the whole of the mouth would become chancreous, that the contiguous glands would become Buboës, that the fluid trickling down the throat would excite ulcers, that the swallowed fluid would ulcerate the stomach, and so very preposterously would the argument upon this principle lead us on, that we must expect, by giving it all its scope, that every secreting surface locally infected will produce Gonorrhœa, and every lymphatic gland a Bubo; every part of the skin will be ulcerated; on every aponeurosis there will be a node; and on every bone a caries.—But if none of these consequences do occur, what temptation have we for reasoning after this manner? We are bound to reason upon symptoms as they do occur, and must confine ourselves to that which we see, and which is the only true nature of the disease.

Notwithstanding we find the latest Authors upon this disease, deciding upon effects of absorption of our own fluids, as if these fluids really possessed a power of feeding the disease. When it is found, on

the contrary, that every venereal symptom, which is a result of constitutional infection, which is a result of a dissemination of original virus received from another subject, is an apparent relief to the constitution. This is evident, by the symptomatic fever, and by manifold debilities which I shall shortly treat upon, being in some measure abated, after the constitution has thrown forth venereal eruptions, and after they have generated into ulcers. And although the constitution cannot by such effects become freed of the foreign venereal poison, yet its existence under that influence is protracted, and the efforts of it are in this instance similar to more general efforts by which it is governed, in endeavouring to free itself of that which is obnoxious to the living principle of it.

If I appeal to the action of other poisons, my argument will not be found to lose strength by such analogy, it will be by that not only strengthened but confirmed. In the Small Pox a violent fever prefaces the eruption, as does a general debility also, and no one eruption in the Small-Pox is generated from the infection of another; nor will matter taken from one pustule, and by wounding a part of the skin, and by applying that matter where there was no pustule, produce another upon the same subject. This experiment I have made.

I therefore infer, that it is the actual absorption alone of foreign venereal fluid imparted by another, which progressively pervades the constitution, which

acts more slowly on some, and more rapidly on others, and which is disseminated through every integral part of the constitution, whenever it be under the whole of the influence of venereal virus.

And in order that this foreign virus should thus be disseminated, I do infer, that it is not absolutely necessary there should be any local first symptom—that it is not to be presumed a constitution may not be as completely possessed of *Lues Venerea*, without any local symptom, as it may be in concurrence with a first local symptom—and that therefore any discharge from the same individual has no relation to the venereal action of that fluid which is absorbed into the constitution, and which was imparted by another. I also infer, that a local symptom upon any part from whence lodged venereal fluid was absorbed, and which in fact was the venereal fluid of another subject, is not a *sine qua non* for venereal infection fully taking effect upon that constitution into which it was after this manner absorbed.

I shall now proceed to give a statement of symptoms which are produced in consequence of the action of this foreign virus, after, through absorption, it has gained possession of the constitution. And also it must not be forgotten, that constitutional symptoms will be most materially affected, and undergo a change, when they have been influenced by mercury. The time of the constitutional appearance of symptoms, after the virus be completely absorbed into the habit, has been one of the most

indefinite and most irregular circumstances of the disease, and I will explain why it must have been so.

When a Chancre or a Bubo was a first symptom, and when the time has been strictly ascertained of the appearance of one or other of them, then venereal symptoms from an infected constitution will follow in regular succession, both in point of order and of time. In general, there will be a symptomatic fever, succeeded by ulcers on the tonsils, with or without eruptions on the skin, in six weeks or two months after the date of either. But when mercury has been imperfectly administered for the cure of local symptoms, the regular succession of constitutional symptoms will be interrupted; or, when the virus of another subject has been absorbed into the constitution, without leaving any local impression, and in consequence of which the patient does not suspect that he is infected from that connection, then ulcers on the tonsils, or eruptions on the skin, will appear within two or three months afterwards. This last possible form of infection being little known, and as little attended to, it has been a general practice to recur back to that period when the patient had a Chancre, Gonorrhœa, or Bubo, and to attribute the cause of the constitutional symptoms to one of them, if it were ever so remote. But whenever I see a patient on the immediate appearance of ulcers on the tonsils, and eruptions on the skin, and who never had any local symptom, I am of opinion, that the disease was contracted by him within three months.

The signs of constitutional infection must be various, as it is found by experience. Some of these signs may be said to be commemorative of the disease, to be assured tokens that the virus is actually present in the constitution, notwithstanding it has not yet avowed itself by any constitutional symptoms. Some commemorative signs not only betray a positive presence of the foreign virus in the constitution, but they tend to confirm those symptoms to be venereal, which, when they appear, might have been said to be equivocal, if such commemorative signs were not taken into consideration.

It has been wisely and truly said by ancient writers—that it is necessary, whoever would form a true prognostick, they should strictly define and properly judge of all the signs, and comparing their forces together, examine which prevails; and there is no instance, in all the round of diseases, when this advice claims more attention, when it can be so applicable as in *Lues Venerea*. Because it will, and it necessarily must produce similar symptoms to other diseases, and which symptoms will be various, from an anatomical variety in the parts on which the disease is liable to fall. The nature of this disease is of so wide an extent, and it comprehends such an infinite number of different symptoms, that it rather appears to be every disease than one disease. Therefore, it were vain to attempt a definition of that which cannot be comprehended within the narrow limit of a definition. It is much better to place, in

successive views, the nature, form, and disposition of the disease, and the series and connection of effects which it produces, by an accurate description and enumeration of its most general symptoms. The whole of the symptoms of Lues Venerea do never occur at one time upon the same subject. The whole of the symptoms never can be produced at once, since some of them are repugnant to others. But it is, notwithstanding, necessary that the whole of them should be known, that they should be placed in successive views, not as they must, from a relative bearing towards each other, absolutely be found upon a subject, but as they might, when any number of them are found together, more clearly establish and point out a disease to be truly venereal. So strongly am I convinced of the power of Lues Venerea, that it exceeds mine, and has exceeded the abilities of any man to follow it through all the mazes which the disease pursues, as if it were designed to harass reason in developing them. For, as the fluids in general will partake of the infection, so will every part sooner or later become influenced by it. As every part is liable to disease from other causes, and as every particular part diseased will have its own particular symptoms and its own particular appearance, necessarily according with the anatomical formation of that part, so will venereal symptoms on every particular part be found to be equivocal with other symptoms from other causes.

It must not therefore be imagined, that the symp-

toms which I shall relate will be all the symptoms that ever happen in Lues Venerea, and that there are no more than will be selected by me: I shall select the chief, and those that are most commonly present, with other more subordinate ones. For if I did otherwise, I should be obliged, not only to go into all the diseases incident to the human body, but likewise into their distinctions. I am well aware, that I am enlarging the extent of power in virus upon the constitution of the subject infected; and that in what I shall hereafter advance, I shall still have in view that perviable power which it possesses of bringing into diseased action any part of the constitution. But it must be remembered always, as I have before now remarked, that I am not introducing any innovation, that I am only confirming those attributes which have been said to belong to the disease by every Author who has given a history of it, from the first account of its introduction, till lately.

Since the publication of Mr. Hunter's Treatise, I do not imagine that those who have not confined their studies to this disease in particular, have recurred to any prior opinion upon it. They therefore will be impressed with an idea, that mine, in comparison with his, is too extreme; and that if he fail of the truth, by narrowing the natural limited power of the disease, I am extending its power beyond that point which the history of true venereal cases will support me in. I shall only ask such, if they believe that the cases of the transplanted teeth are truly venereal? if the diseased Iris be ever venereal?

and if they believe that the accumulated opinions of former Authors are also true? I know, as well as any man who reflects can know, that the prejudice from first impressions is hard to be done away—that the mind must be prepared for such a revolution, must be engaged *de novo*—and that a change in opinion is not the sudden result of a stronger argument—but that the mind must be disencumbered of a wrong habit of thinking, before it can be free and open for receiving the impression of a right one. The most energetic mind that ever was possessed by man is capable of being imposed upon, is capable of receiving an impression from early prejudice, which, like a weed in a rich soil, will root more deep, and grow more rank.

I therefore know, that I am not only to affirm my facts, and to impress those with them who are open to receive them, but I am also to take off from others the prejudice of a wrong theory, and to invite them, from habitual reflection, to examine into the merits of a true one, before they will be prepared for receiving it, and firmly adopting it. It is necessary, from that motive, for me to assert, that by observations which I have made from cases upon the nature of the disease, as well as by that strength which my opinion derives from the authorities of others, that there is not a visible part of the constitution which is not exposed to affections from *Lues Venerea*.

I do not intend to inquire farther, how *Lues Venerea* gets into the constitution, by being absorbed from that part to which the venereal fluid, received

from another first adhered, as that point has been, I flatter myself, already fully discussed, and strongly exemplified. It will be sufficient for me now to state, that there are two ways by which the infection is communicated to the habit. The most ordinary and common way is by the absorbent vessels, and through which the disease is communicated to the blood. The most extraordinary and uncommon way is by the infection being immediately communicated to the blood, and without the intervention of absorbent vessels. By the ordinary way is meant, that natural intercourse of the sexes through which it is produced; and by the extraordinary, that natural consequence which, as I have shewn, might produce it from a transplanted tooth vitiated by venereal infection, or any thing like it, as by inoculation, &c. &c. In the former, it has to pass through the system of absorbing vessels into the blood; and in the latter, it is communicated more directly to the blood, from not passing by lymphatic circulation. In the former, its consequences are slow, and appear at a greater and more uncertain distance of time; in the latter, its consequences are more rapid, more violent, and to a certainty in a much shorter time, the whole of the leading features of the disease being particularly strong and decisive.

Although it is manifest from experience, that the whole of the fluids (as they hold a communication with the blood in every relative sense) must be infected when the blood is infected, yet it will be seen

that the venereal poison affects some fluids sooner, or more apparently, than others, that it more readily produces undeniable symptoms of its influence upon them, and that it more visibly betrays its action on some particular parts than on others. And although any part is liable to be vitiated from other causes, and the evident morbid state of it cannot be ascertained to be positively venereal by one instance alone, yet there may be apparent symptoms, at the same time, on other parts, and from comparing them together, they will confirm a disease to be venereal. Consequently such fluids, and such parts, which from their anatomical construction have the strongest disposition for first attaching the disease to them, will be most in common liable to betray the actual presence of it in the constitution, by their affections. I shall proceed in an enumeration of the most leading, and the strongest signs, which both indicate the disease to be venereal, and by which we are to know it to be venereal.

I have already hinted at a sign of the presence of the disease, before the constitution has betrayed it, and which I term a commemorative sign : it is, when a local part, which was the part to which the virus was imparted from the infecting subject, is not restored to a perfect sound state ; when, in consequence of a Chancre, for example, the part where it was seated is thickened, and when there is in it a disposition to ulcerate afresh ; when the lymphatic glands of the groin are enlarged, and when they are

tender, or not, to the touch; or when they are swelled beyond their natural proportion in a state of prior soundness. These are strong commemorative symptoms of the disease being in the constitution; as are also redundant discharges from the sebaceous glands behind the glans Penis, and excoriations, and increasing warty excrescences. If equivocal symptoms of venereal infection come forth from the constitution, and in the presence of these commemorative symptoms, the conclusion must be, that such equivocal symptoms are truly venereal. It is not always certain, that these commemorative symptoms will be present when the disease appears; for I have already said, that sometimes the absorbents take up the deposited virulent fluid, without there being any local impression left upon the part where it was deposited: but this is rare, and even when it happens I am doubtful whether or not the glands of the groin, if they were watched, would not betray the escape of the virus into the constitution.

Every subject, when locally infected, is attempted to be cured by more or less of that mineral which HAS BEEN universally given in this disease. And from this cause, the action of venereal poison, when it is not completely extinguished, may be, and is for a time only enfeebled, and the influence natural to it thereby protracted. A longer time will be in consequence afforded for the disease to diffuse its baneful effects, for it to renovate and collect again in force, for it, by long uninterrupted exertion, to demon-

strate that power it fully possesses, and for it to come forth with all its true symptoms. When the operation of mercury has been unsuccessful, when the application of it has been injudicious, the action of the poison might have been only suspended, but not destroyed; the snake might have been scotched, but not killed; the disease then will be certain to be betrayed by the effects of it, if not so regular in point of time, yet as true in point of symptoms and action. I shall to these causes attribute that tardiness in its appearance which is found to prevail more now than it did on the first appearance of the disease in Europe.

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DEFINITION OF PRIMARY SYMPTOMS OF LUES
VENEREA FROM AN INFECTED CONSTITUTION.

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Prior to the appearance of cutaneous eruptions, from venereal infection, the patient feels an uncommon depression of spirits, and a languor that cannot be described. He feels erratic pains on every part of his body, and an aching pain in his cylindrical bones darting through them from without inwards; and he feels also frequently a pain on the pericranium, as if it were bound tight upon the bones of the head. When these pains are not severe in the night, they generally cause restless tossings and inquietude. These seem to be very different from the excruciating and boring pains which attack cylindrical

bones in long habitual venereal infection, and which constantly thicken the periosteum of them. The former pains may be said to be merely erratic, and to be confined entirely to the periosteum, the muscular, aponeurotic, and ligamentous surfaces. They are sometimes so light as scarcely to excite a complaint about them, but at the severest they are evidently milder than the other species. A languor and lassitude are not only experienced during the day, when the patient is up, but are more experienced in the morning after rising, the sleep which he had, and the bed on which he lay, affording him neither enjoyment nor refreshment. The fever which accompanies these symptoms is not of the inflammatory class; the pulse is quick, the tongue is streaky, the shoulders are sore, the small of the back is aching, and the patient evidently wastes.

These symptoms preface ulcers on the tonsils, as well as eruptions on the skin. The more general and complete these eruptions follow, the more apparent is the remission of the pains, and the abatement of the other foregoing symptoms. The whole of the complexion of the skin will be found to be changed to a tawny hue. Plain spots appear, not protuberant, especially upon the breast, and upon both shoulders, of a red colour, purple, yellow, or livid; sometimes distinct, small, circular; sometimes broad, and spreading wide. They appear frequently in the hair, with a scab on the forehead and on the cheeks, dry, running, furfurose, and frequently like

an herpes, and also deep and ulcerating through the true skin, making a large cavernous ulcer on the forehead, which is called a corona veneris. In the palms of the hands, and the soles of the feet, these eruptions will generate into clefts, which will become hard, callous, itching, and discharge a thin ichor, and the cuticle being loosened from the subjacent skin, separates from it in small pieces like scales. These spots will also deform the skin, with hard, callous, circular tubercles or pustules, not rising very high, ulcerating at the top, for the most part dry and without matter, but sometimes moist and running, scaly, furfurose, and yellow. They are common in the corners of the mouth, and on the alæ of the nose; and this sort is frequently found about the forehead, the temples, upon the hands, the wrists, and upon the thighs and the buttocks, and upon the loins; and sometimes they are dispersed all over the body.

Other primary symptoms are, an affection of the tonsils, and other internal parts of the mouth, throat, and nose. The uvula, tonsils, and the whole arch of the fauces, suffer pain, heat, and inflammation, are eroded by ulcers, and a caries is communicated to the bones of the palate, by which they are in a very short time destroyed. Tubercles and pustules arise in the palate, which degenerate into round phagadenic ulcers, by which the whole of the bony roof of the palate is perforated with a caries as far as the nose. The membrana pituitaria, partaking of dis-

ease, either is thickened, is fungous, ulcerous, or callous, terminating in ozæna or malignant ulcers, from which the spongy bones, and the two triangular bones of the nose, with the vomer by which it is supported, becoming a caries, sink. This is the cause of that manifest depression of the nose from venereal infection. The speech in consequence becomes changed, the voice is hoarse, and sometimes the subject cannot be ever after understood, unless lint, or something else be stuffed in to supply the vacuum. The gums will be eroded, and the teeth will drop out, the breath will be foetid, and hot and raking. As the affections of the skin and tonsils, and sometimes the palate, but not always, for these appearances will there be generally later, are synchronous, I thought it fitting that they should be mentioned together. But those of the palate would soon succeed the former, if they were not prevented by mercury. The ulcers on the uvula will have at first a white appearance, as if a piece of hog's lard were lodged upon the part, which in time will erode deeper and deeper; whilst those on the tonsils will be deep, and, in their appearance, apthous. To the eye that is conversant in observing them, they can *hardly fail of being well defined*, though not so easily described; but mercury will make always an alteration in them. Thus much I can proclaim, without hesitation, that whenever the tonsils and the uvula are ulcerated from a venereal cause, the ulcers will never spontaneously heal, but will continually spread, if that be not prevented by mercury.

OTHER VENEREAL CONSTITUTIONAL SYMPTOMS AT A MORE ADVANCED STAGE OF LUES VENEREA.

By recurring back to ancient Authors, the constitutional symptoms will be found to be defined exactly correspondent with the present action of the virus, whenever it be truly left to exert it. Thus VIGO, after he has treated upon a Chancre; proceeds in a relation of the following symptoms, in consequence of the foreign virus having been absorbed into the habit. It is found in his *Practica Copiosa in Arte Chirurgica*.

“ Quamobrem non ita hujusmodi pustulæ beneficio, medicinali, intus et extra applicato, poterant à medico curari, ut non semper earum malitiâ totum corpus amplexaretur, cum ulceratione partium genitalium difficillimæ curationis et facilis recidivæ: quare, licet predictæ pustulæ deponantur, tamen etiam atque etiam solent recidivam facere: quare post earum sanationem quamprimum pustulæ crustosæ et interdum cum carnositate elevata ad modum verrucæ super frontem, caput, collum, et faciem, brachia et tibias, et fere per totum corpus diffusæ fuerant: et usque in hodiernum diem hujusmodi morbus hunc ordinem servat. Similiter quoque interdum cum dictis pustulis, vel saltem post earum ortum, inde ad mensem cum dimidio vel ultra pa-

tiens laboravit, doloribus clamosis interdum circa frontem, interdum in scapulis, et humeris et brachiis, et aliquando in tibiis, coxis et anchis. Quibus doloribus, aliquando multum post tempus, viz. post annum et ultra, quædam oriebantur schirrhositates ad instar osseum; à quibus ægotantes doloribus clamosis vexabantur presertim tempore noctis, et sedabantur die. Pro quorum sedatione anodynæ medicinæ à doctoribus commemoratæ sedativæ doloris administratæ nullum affectum doloris sedativum ægotanti præstabant. Cujus doloris finis ferè semper fuit, os, et almoschatin corrumpere et vitiare quemadmodum in ventositate spinæ accidit."

This history of the disease was given by VIGO, in 1513; and thus it appears, that as it was at first, so it now is, not on the decline, but exactly the same. Eruptions then, from a constitutional infection imparted by a Chancre, were produced all over the skin, in about six weeks. These were preceded by acute and erratic pains; and in a few months afterwards, these pains became more and more intolerable, and particularly at night; they then attacked the bones, and made them caries. If I were to rest the present definition of symptoms of the disease upon the faithful report of ancient Authors, perhaps I might not be so likely to be misled. For although I am as anxious after the truth as they could have been, and although I defy the preferable purity of any man's motives, yet their opportunities were more favourable than mine. For VIGO was

among the first who introduced mercury: he therefore, and others before him, must have frequently seen the natural symptoms and progress of the disease when not disguised by mercury. It appears from the concurring testimony of every Author, that nodes and caries bones were the latest symptoms of the disease: and that it was the same formerly as it is now, in that respect, I have shewn from VIGO, who ends his description with symptoms of its attack on these parts; as does also FRACASTORIUS, in his treatise de Morbo Gallico, whose description of constitutional venereal symptoms I will also produce.

“ Tandem, quod in majori parte inerat, ulcuscula quædam circa pudenda oriebantur, iis non dissimilia, quæ solent ex fatigatione contingere, quam, curiam vocant; sed natura longè impar, nam hæc et emori contumax erat, et victa una parte, alia regerminabat immortali propagine. Post hæc crustosæ quædam pustulæ per cutem erumpebant, in quibusdam quidem à calvariâ incipientes (quod ut plurimum erat), in quibusdam in aliis locis parvæ primum eæ apparebant, mox augebantur paulatim ad magnitudinem cooperculi glandis, et similitudinem etiam iis non adsimiles quæ in pueris achores vocantur. Differentiæ earum multæ visebantur, quibusdam parvæ et sicciore, quibusdam majores et pinguiore, nonnullis lividæ, aliis exalbidæ leviter pallentes, aliis duriores et subrubentes. Omnes autem paucis post diebus aperiebantur, ac mucore

quodam mucilagineo fœtido manabant, nec dici potest quantus ille mucor perpetuo affluebat, quanta sordities. Exulceratæ deinde exedebant more eorum ulcerum quæ phagædenica appellantur, atque interdum non solas carnes sed et ossa etiam ipsa inficiebant."

From what I have here quoted, and from what constantly has occurred, in the most general sense, in this disease, it appears that the bones are the latest parts affected: and I flatter myself that I shall not be found to depart from the truth, when I lay down some few positions, which are not only confirmed by the history of the disease, as it was described by our ancestors, but also by the appearances of it, as it ordinarily occurs at the present period.

A languor, prostration of bodily strength, quick pulse, erratic pains, loss of appetite, restless nights, wasting of the habit, and an undescribable change in the countenance, precede the eruptive state. The eruptions generally appear in six weeks after the habit is tainted, they are different in their appearances on different parts of the body, and on some they now and then disappear spontaneously, leaving their mark behind them. There will be a constant succession of them, and they will in time generate into ulcers. Ulcers on the tonsils, and on the palate and nose, will go on to destroy every part to which they are attached. In some time after, the pains will become more fixed, tormenting and

constant ; particularly at night time, the rest will be broken, and the disease will then begin to shew itself by nodes on the periosteum, by gummata on the aponeurosis of muscles, and after these a caries of the cylindrical bones and of the head will follow. The softer and most sanguineous parts become the first victims, and so on to the harder, which will be the last to be attacked, when the disease has in process of time collected in force, and is enabled to vanquish obstacles, which in the beginning it could not effect. For the erratic pains which precede every appearance, may be considered as an effort in the constitution to get rid of the disease, and as an effort in the disease to obtain a lodgment: the softer parts, being with more facility overcome, more abounding with blood-vessels than tendonous and bony substances, are consequently the first to experience the effect of poisonous action.

The order in which the disease appears is accounted for from the different construction of the parts. They are all from the first equally exposed to be attacked by the disease, and it is not improbable that it is acting on every part, from the very beginning of the primary constitutional symptoms. For, as the periosteum and tendons betray signs of disease by nodes, in some months, which signs are betrayed on sanguineous and softer parts, by ulcers, in six weeks, so by comparing these different constructions, it is natural to infer, that such would be the effect if the date of original diseased action on

every one of them were the same. Besides, it must be remarked, that the pain, which is so intense in the end, comes gradually on the periosteum and tendons; that by their resistance to extension and thickening, the pain is created, and that the disease must have made already some progress before such effects could have been produced. I am, therefore, strongly inclined to believe, that the disease on these parts commences with other earlier appearances; for, as I first remarked, I have seen them come on from a malignant and spreading Chancre, in six weeks after the Chancre healed, and they never shifted, but continued to increase for nearly twenty months, until the patient was cured by mercury in extraordinary quantities, and by many repetitions. The pains at first were somewhat erratic; but what I mean to say is, that from the first attack of these pains, which was six weeks after that the Chancre healed, for twenty months the patient had no interval of rest, nor had he any other symptom but nodes, and was unremittingly bored by these pains. All this can be very readily accounted for:—He took at times enough of mercury for preventing a return of the symptoms on the softer parts, and which usually precede the pains and nodes on the tendons and periosteum, but he took not enough to eradicate and extinguish the venereal action on these parts, and on the constitution in general.

In order to prove that my theory of the disease commencing on the harder parts at the same

period that it commences on the softer is true, and that its protraction is only in consequence of mercury, I shall give another quotation from Fracastorius :

“ Præter prædicta omnia, quasi parva illa forent, ingentes lacertorum dolores accedebant, sæpe cum ipsis pustulis, interdum ante, nonnunquam post, et ipsi quidem diuturni ; quibus nihil crudelius aderat : affligebant præcipuè noctu, dolor autem non propriè in juncturis inerat, sed circa lacertos ipsos et nervos. Verum quibusdam, nihilominus, sine dolore ullo oriebantur pustulæ ; quibusdam, sine pustulis dolores ; major pars utriusque affligebatur.”

I do therefore repeat, that in common these symptoms of the disease may remain after mercury has been given effectually for removing symptoms on the softer parts, but ineffectually for the prevention of those on the harder, and that this is generally the cause of their appearing, and continuing, when no other symptoms are apparent. And I do also repeat, and my authority for it is corroborated by this quotation from Fracastorius, that the rudiments of these symptoms commence with those on the softer parts, and that it is only from a difference in the anatomical construction of them, which makes the difference in point of time in their appearance.

The disease, when it shows itself on the aponeurosis of muscular parts, produces a tumour which is at first hard, and which often afterwards becomes soft, containing within it a glary mucus ; the part

will be exquisitely tender at the touch, and will, in process of time, generate into a spreading cavernous ulcer. The ligaments and tendons on the joints often suffer from this cause, and the pain is like that in the gout, from its being produced on the same parts. When the pain comes on upon the periosteum of a cylindrical bone, it will be found to extend the whole length of it, and to be only confined by each epiphysis of the bone. There will be exquisite tenderness experienced on touching the part, the periosteum will be thickened, and the skin which covers it will be œdematous, retaining the impression of the finger. The periosteum will at length inflame the skin, it will ulcerate, and sometimes slough away, exposing thereby a caries of the bone to view. But when this is not the case, and it is not commonly so, owing to the disease not being permitted to proceed, the node will be found to be hard and circumscribed, extending itself up and down the surface of the bone, and sometimes there will be distinct nodes on the same bone.

The ischiatic parts will be often materially affected; they will become enlarged, and the whole of the external part of the hip will be painful. A lumbago will also affect the loins. The pains which affect the bones and the membranes will be such as if they were continually boring through by a trepan. The bones will be subject to the worst effects, when the disease has been suffered to go on, and when the power of it has been long continued so as to produce

them : the thickest parts of them will swell out into greater or lesser exostoses, sometimes with soft excrescences, and sometimes with hard ; and these at certain times will excite clamorous and raging torture ; at other times they will give the patient very little trouble, just as the parts in succession are changing from a state of health to that of disease. The bones at their extremities, where they are spongy, and less compact, will by degrees be enlarged, with a slow accretion of matter, through their whole substance, but unequally, according to an inequality in their thickness. This is owing to the periosteum having been previously destroyed. The pain that is experienced will not be so intense as on harder parts of bones ; and after they are enlarged, there will be an ankylosis. A caries will penetrate the whole substance of the bones ; it will be found to affect the medullium, as well as the superficies, and to be so universal as to destroy the living principle of them ; from whence they become incapable of bearing the least force, and are broken without the least, in the end.

The lymphatic secretions, and the glands that secrete them, also experience the ravages of the disease. The eye-lids will be thickened, rough, red, itching, watery, callous, and ulcerous ; the tunica conjunctiva will appear œdematous, inflamed, watery, and ulcerous, discharging continually a sharp acrid fluid ; and the cornea will become opaque, and sometimes it will be ulcerated. The

very humours will be vitiated, and vision will in consequence be destroyed. The caruncle situated in the greater canthus of the eye will be much enlarged; and the saccus lachrymalis will be ulcerated, producing by it a venereal fistula lachrymalis.

The Iris also is exposed to the infection of Lues Venerea*. A pale redness all round the cornea is

* The Quarterly Journal of Foreign Medicine and Surgery, 1st November, 1818, has given a most excellent paper on IRITIS, from Professor SCHMIDT of VIENNA, who was the first both accurately to discriminate the disease from the VARIOUS KINDS OF OPHTHALMIA with which it was previously confounded, and to GIVE IT THE NAME OF IRITIS.—It is surprising, he adds, and so it is to me, that this important Essay has not been hitherto noticed in England. I need not add to this note, that lately a paper on IRITIS has been published by Mr. TRAVERS, and that nothing has been said in that paper, about Mr. PROFESSOR SCHMIDT, WHO WAS THE FIRST PUBLISHER ON THE DISEASE, AND WHO GAVE THE NAME OF IRITIS TO IT. I should also remark, that in the Second Edition, where Mr. Travers's Paper appears, nothing is there said of the AUTHOR of IRITIS. Professor Schmidt's publication came out in 1801. Mr. Travers has noted Dr. Beer's Work, which was published in 1813, who describes, as distinct affections, a venereal and arthritic IRITIS. The opinion of Mr. Travers, is to confound the action of mercury for the cure of IRITIS, by a sort of equivocate: it brings it on and it cures it. Mr. Travers has said, that the beneficial effects of mercury is an observation but of a few years date, and there he refers to Dr. Beer of Vienna, in the year 1813: this is exactly like to what John Hunter would have said. The fact is, that Professor Schmidt published his Essay in 1801. There is nothing in Mr. Travers not to be found to the purpose in Professor Schmidt. BOTH INSIST THAT LUES VENEREA IS A CAUSE OF IRITIS, AND

the first symptom perceived ; this is at first seated in the sclerotic alone, but the conjunctiva very soon shares in it, and afterwards becomes much the more red of the two. However few the vessels may be elsewhere, there is always a broad zone of them all round the cornea, a zone formed at this place not on' by the vascular net-work in the conjunctiva, but by the ciliary vessels on the external surface of the sclerotic. The redness has a peculiar tint, for instead of being bright red it is brownish, something like the colour of cinnamon. From this zone the vessels have a tendency to be prolonged

THAT IS ALL I WANT FOR MY PRESENT ARGUMENT. Having said thus much, I wish to say something more as to Professor Schmidt, and which is, whether he be now living or not, I wonder that any author of an English publication, coming forth to the world with such high names (and one of their Essays bearing the title of the author who christened it), and avowing that the beneficial effects of mercury in the cure of it IS AN OBSERVATION OF LATE DATE, should not have paid some attention to the name, rank, and reputation, of such an author as Professor Schmidt of Vienna ! I must confess, I feel extremely earnest upon this head, because I think the English body of Surgeons are interested in the explanation. If any one of us went to Vienna after the second edition of these Essays appeared, and talked of the names of Schmidt and of Travers, we should feel ashamed of the omission ; and therefore I recommend to Mr. Travers, when the Essays come into a third edition, to explain the cause of the omission ; for I am confident, from the known honour of the parties, and from their anxious desire that every bird should have his own proper feather, that they will do so, and that they will do it after such a manner that an English medical man can travel over the continent with credit.

under the edge of the cornea. The whole cornea now becomes uniformly hazy, losing its clearness without being in any place actually untransparent. This appearance of the cornea seems dependant on some affection of its posterior surface, or, more accurately speaking, of the membrane of the aqueous humour, by which it is lined. The pupil becomes contracted, and the iris limited in its motions, as in common iritis ; but the pupil does not preserve its natural situation. It is removed in a direction upwards and inwards, towards the root of the nose, and is irregular. Along with this, the iris loses its natural colour, and projects forward.

There is always an aggravation in the symptoms towards evening ; the intolerance of light and painful sensibility of the whole eye increasing, and a gush of tears following every change of light and temperature. At length a regular nightly pain sets in, is extremely severe, and is strictly limited to that part of the cranium immediately above the eyebrow. It usually begins between six and seven in the evening, gradually increases, reaches its utmost height about midnight, and then diminishes, till about four or five in the morning, when it ceases. After every such attack of pain the pupil is found more contracted, drawn farther upwards and inwards, the iris more altered both in colour and form, the quantity of lymph increased, and consequently vision more impeded.

The ears also will not be found to escape. There is not a more common, manifest, indicative symptom of venereal affection, and especially when associated with other affections, than an hissing noise, an aching pain, thickness in hearing, and deafness. The internal part of the ear, the cavity of the tympanum, the mastoid sinus, the canals, and the labyrinth, are exposed to suffer by inflammation and ulceration, attended with violent and continual pains. The small bones which assist in the action of the ears, and the bony arch of the ears, will all be liable to be destroyed by a caries. A sanious ichor will flow from the ears, as from a fistulous sinus : the cartilage of the ears will be thickened externally.

The functions, as well as the parts which produce them, become impaired or destroyed, by which a period is put to the existence of the patient, sometimes by colliquative fever. A wasting in the solids will expose the glandular parts, which will be found to be enlarged and knotty. The secretions every where will proclaim the disease : as will also ragged excrescences on the verge of the anus, thickening of the prepuce, redundancy of discharge from behind the glans, encreasing warts, scirrhus of the testicle, thickening of the spermatic chord, ulcers on the scrotum, falling off of the hair and eye-brows. The nails will be diseased, lose their shining appearance, and will become rough and uneven ; and there will be a redness on the skin at their roots. Such are the

general effects of venereal infection. In short, the whole of the fluids and the whole of the solids will be found to undergo a diseased change, from venereal constitutional infection. It will be apparent, by the known progress of the disease, and by its universal action, that sooner or later every visible part of the body, the whole of the solids, and the whole of the fluids will be exposed to suffer, to be changed, and to be contaminated by it.

I have related such symptoms which are certain to be the effects of venereal infection, and which will progressively go on, without an intervention of mercury. But it must not be imagined that any relation which is or can be given of the venereal poison, will be perfect. Every patient who is confirmedly under the influence of venereal poison, will be possessed with some of these leading symptoms in the disease, but the whole of these symptoms are scarcely to be expected upon the same subject; nor will the desperate ends of the most important be seen, but when the disease has gone on, from error in judgment of its true nature, or from its not having been treated in any way. No point in practice ought more to be inculcated, than care in ascertaining the true nature of any disease, in the first instance; for if a diagnostick be wrong, nothing after, that can be done, will be right. A Surgeon perhaps might conceive that the pains which attend venereal affections of the bones and periosteum were from rheumatism, and this wrong definition might

be never corrected. The patient will never, in consequence of treatment for rheumatism, experience ease, nor obtain a cure ; but on the contrary, his disease, under that misconception of it, will proceed, will become more and more aggravated in its symptoms, and the torments from it must be as long endured by him as the true nature of it lies concealed.

Irregularity, and impatience in submitting to regimen, will be a cause of Lues Venerea being left behind after the use of mercury. And this will happen when a Surgeon is chosen particularly for his reputed intuitive adroitness : for although he can always answer for his own sound opinions in practice, yet he cannot be responsible for the event of instructions which are dictated with a certain probability of advantage, and which are absolutely necessary for securing the safety of the patient from future return of symptoms, if such advice be imprudently neglected, or only carelessly followed. This is a cause of most anomalous symptoms. The interval between an original complaint and a return of it may be a long time, when mercury has been used but ineffectually. Many are the instances which may be brought to prove a recurrence of venereal symptoms, year after year, when the disease has been imperfectly cured. Such interval between original infection and the recurrence of symptoms may not, to a common observer, betray the least sign of any remaining infection ; the latent sparks of it, when unextinguished, may not directly

rekindle, and collect sufficiently for discovering a true cause by which they were produced.

I would not advise any person to imagine, that he must be free from a venereal taint because no symptoms directly appear after an irregular use of mercury : for it must not be forgotten, that although all the symptoms which I have enumerated be strong affirmative proofs of the action of the disease, whenever they appear, yet an absence of them is but a very weak negative proof of the constitution being entirely free of the disease.

When the disease has frequently occurred, and at very distant periods, it will become so assimilated and habituated with every part of the constitution, that it is scarcely possible to eradicate it. But if, by a quantity of mercury, and that continued long, the disease should yield, and the symptoms of it retire, a knowledge of that effect ought to be the strongest inducement for encouraging a perseverance, from time to time, in that remedy, which at once betrays the disease it is intended to cure. It seems impossible to reflect, without horror, upon the miserable lot of that subject who is possessed of this disease, but who is not impressed with a true idea of the nature of it ; who appeals for relief, but can obtain none ; and who flies from remedy to remedy, from want of the original cause of it being detected.

The attention should ever be awake to a possibility in recurring symptoms ; for if that be not the

case, the most certain venereal symptoms that ever were defined may be overlooked, and may be treated after every other manner but as venereal. The whole of the *Materia Medica* will then be in vain appealed to for effecting a cure. This possibility, by those who are the most conversant in the nature of the disease, is by them the most strongly impressed. And it therefore should be never forgotten, that if a disease does not yield to all other powerful remedies, and that if a disease has the least similarity in symptoms with those which may be not only defined true venereal symptoms, but also with equivocal symptoms, such a disease might be a recurrent *Lues Venerea*.

The importance of this question, I must repeat it, is very high; for if it be taken for granted that the case be not venereal, even when the disease and the possibility of it has been recollected, but otherwise decided and otherwise treated, and when at the same time the disease was venereal, then the patient will be shut out of every prospect before him but that of a diseased and miserable existence. To prove this argument, I could instance the mistaken proceedings of many cases, and the events of them. It will be extremely necessary to attend to this circumstance of *Lues Venerea*, and to be awake to the possibility of it. The importance of knowing this fact, and of giving it its due weight, will be found very essential. Such cases, rightly defined, will gain more credit than those which occur

early, and at regular periods, from the first local infection; for recent symptoms carry with them no suspicion of the disease being any other but venereal. Such speak for themselves, and plead their own cause. But when the fact is doubted, a cure, through a long course of mercury, is submitted to, from want of confidence, with great reluctance.

An Officer, when quartered in Ireland, was attacked with a Chancre more than two years before I saw him. His Chancre was healed, in consequence of mercury; but his mouth becoming sore of a sudden, mercury was left off while the Chancre was healing. A hardness and thickening of the parts remained, and there was a second tendency on the chancrous part to open afresh. In a short time afterwards it opened afresh, and he was attacked with pains, which were truly erratic pains, such as precede eruptions. Eruptions soon followed, in consequence of which he applied to mercury again, and his mouth soon became sore. Because his Surgeon and Physician judged that he should have enough of mercury, they rubbed in near an ounce at once of ointment, made of equal parts. His mouth again being thus suddenly made sore, he desisted from mercury, and his pains disappeared, as did also the eruptions, one of which had left a large scar on his forehead, and another below his left eye, on his cheek. In about twelve months after this, his pains returned, and arrived to such a pitch, that he had no remission of them at night.

He had gummata on his elbow, and nodes on his tibiæ. He was emaciated. He had an ulcer just within sight, behind his uvula. And moreover than this, he had a considerable node on one of his parietal bones.

Now I beg to know, how it was possible that this case could be mistaken? But the sequel of the case will prove not only that it was mistaken, but how it was mistaken.

The Surgeon and Physician thought they had done enough with mercury, and the Officer, despairing of a cure there, with difficulty reached London last August was twelve months, and applied to another Surgeon. That Surgeon sent him down to the salt water. He bathed in both hot and cold baths, and took bark, vitriolic acid, and opium at night. After remaining there for six weeks, he returned to town, and was but just capable of crawling with the assistance of crutches. He paid his Surgeon a second visit, who then told him there was no farther call for mercury, and who prescribed for him increased doses of opium, and gum guaiacum. The Officer finding that his case was no longer considered as venereal, and from that cause, judging he could, from the practice of a Physician, obtain that relief which he had hitherto sought in vain from his Surgeon, the Doctor, upon hearing the opinion of his former Surgeon, prescribed bark and steel for the case. But on his second visit, he told him of his suspicions that the disease was

yet most likely to be venereal, and desired him to consult a Surgeon again. It was at this period that I first saw him. After hearing his case attentively, I told him, that I could do nothing for him, until my opinion were confirmed by another Surgeon, and we were both confirmed in the opinion, that the case was venereal. The symptoms then were those which I have already stated. The result of this case, in a few words, was,—that by a gradual introduction of mercury for two months, the patient became quite restored to health; whereas, if mercury had been hurried into the habit, that restoration to health would never have been obtained. In this consisted the whole of the difference—a difference which in its consequences was very serious and alarming.

I will make but one remark. If in this palpable case every possible idea of its being venereal was renounced, how much more might it have been so, if the symptoms had been more anomalous. The only staggering circumstance in the case was, that the ulcers on the forehead and cheek had healed. This proves, that symptoms on soft parts will give way, when pains in the bones do not, and that with less mercury success will be obtained in the former instances than in the latter. I do not call this an intricate anomalous case, nor any thing so equivocal; but if in such a case as this, the true disease be left undecided, what will be the fate of those infected, whose symptoms are more intricate and anomalous?

I attended an irregular lady, who was attacked by boring fixed pains in the bones of her legs and arms. The periosteum was thickened on the tibiæ and radii. She rubbed in mercurial ointment, four times, at four different periods. Each time she rubbed in, the pains relaxed, and the thickening went down: the last mercurial process, by being made more protracted, and by her being closely confined to her chamber, completely removed her complaints.

I was applied to, to visit a lady who came from the North of England, to be advised for a fixed pain which she had on both her tibiæ, but more especially one of them was uncommonly painful at night time. The skin was œdematous, and the part was so exquisitely tender, that she could not bear the bed clothes. I knew her history—that she had been in promiscuous venery in this town, that she had been married three years, and that she had ever since privately lived in the country, and had conducted herself exemplarily sober, and in every sense properly. I found no difficulty in persuading either her or the husband into a belief of my opinion of the nature of her case. I understood that every antispasmodic had been long tried, that she had endured pains for twelve months, that they increased every night, and that they were always most boring in wet weather. I gradually insinuated mercury into the habit. In a fortnight her pains began to abate; in a month she could bear the parts to be touched; in two months only a little hardness and

thickening remained on the periosteum of the tibiæ; and in three months I desisted from mercury, which had been continued daily to that time, during which she positively grew better in her appearance of health, recovered her flesh, and to my knowledge she has remained perfectly well.

It was the state of her husband, which first awoke a suspicion of the case of this lady being venereal. He, from being an healthful handsome subject, had grown thin, had a nauseous discharge from behind his prepuce and which was thickened, had ulcers on his tonsils, eruptions on his skin, and two nodes on his forehead. For these appearances he had been for some time taking advertised drops, and which, it may be presumed, had made the disease so long stationary. He submitted to a mercurial process, after a very regular manner, and also got well. The manner in which he was infected was most assuredly by the venereal vitiated secretions from his wife.

From the present mode of treatment of eruptions which have followed Chancres healed without mercury, I anticipate increased numbers of cases of children born with venereal infection.

A young gentleman, just before he married, had been attended for venereal symptoms of an infected constitution. Thinking himself safe, he married a beautiful woman, who was delivered of a boy at the end of ten months free of all venereal infection. During her second pregnancy, the young husband declined visibly in his state of health; and, within

five months of the second delivery, I found him with a shade over his eyes, which, upon inspection, were both of them inflamed. There was but one more suspicious symptom, which was a fungous excrescence round the sphincter ani. Being delicate about the avowal of the true nature of his case, he consulted an oculist for his eyes, and yielded to the taking of solution of sublimate in sarsaparilla, and some hydragryrum nitratum was applied to the fungous excrescence round the sphincter ani. All the symptoms giving way to these remedies, he did not, unfortunately for him, persevere in them long enough to make himself secure. At the very time the wife was delivered of a female child, I saw the young husband again, in a worse state than ever, for there was one vast phagadenic ulcer spreading from the palate to the lower part of the Larynx. The case becoming thus serious; he submitted to a perseverance in mercury; and after some exfoliations had come away, without any disfigurement of his face, in four months he got well; but he was under the necessity of wearing a plate adapted to the palate to assist his articulation. In four months after his recovery he bathed at Ramsgate, and was seized with a brain fever, which terminated fatally.

During my attendance on the young husband, I was desired to look at the infant, which evidently demonstrated the strongest symptoms of venereal infection. It was squalid, and full of eruptions scattered from head to foot. It seemed to swallow with

difficulty, and it could not, although desirous for it, suck, which happened to be fortunate in a cure of the infant, as it could not suffer by weaning. The child lived upon goat's milk. Its nose was so stuffed, that when it attempted to suck it could not draw its breath.

Having the confidence of the mother, I found not the smallest symptom of venereal infection on her. And when I told her the state of the infant, for she knew the state of her husband, she seemed pleased that by her not having any symptom of the disease, the practitioner of midwifery who delivered her would not discover the state of the family, and therefore the infant was consigned to my care. By rubbing ten grains of unguentum hydrargyrum on the soles of the feet every night, the system could be continued without interruption. The infant most rapidly amended, ultimately appeared free of all symptoms some time before the mercury was left off, and has continued as well as if there had not been any infection. This is a case which shews that the infant was not infected in its passage of birth.

CHAPTER XI.

HISTORY OF REMEDIES FOR THE CURE
OF LUES VENEREA.

MERCURY had been applied to the purposes of medicine by Arabian Physicians, prior to that knowledge of its use amongst us. Rhazis, who was the Arabian Author that gave all the insight of the practice of the East to European Physicians, recommended an ointment in which quicksilver was an ingredient, for the cure of cutaneous eruptions. It was from analogy to this, that Vigo, Berengarius Carpus, and Fallopius, with other Physicians, who practised at the time Lues Venerea appeared in Europe, first tried the effect of mercury, in the form of an ointment and in the form of a plaster, for the cure of cutaneous eruptions in consequence of venereal infection.—These eminent authors on Lues Venerea did not take the idea immediately from the Arabian Physician, and were not the first who used the ointment for cutaneous purposes. That idea of Rhazis was already in practice, and it was from analogy in its curing eruptions from other causes, that they were induced to try it for cutaneous venereal eruptions.

Nothing proves more incontestibly the fact—that Lues Venerea was a disease unknown to European

inhabitants before the return of Columbus from the Western Islands — than the consternation which is avowed by the Physicians of that time, and the ignorance and the despair which they in general deplored, when they first began to cure this new disease. For in the language of one of them, in the year 1500, “The learned avoided having to do with this disease, frankly confessing that they knew nothing about it; for in our days there is nobody who had ever seen such a monstrous complaint; therefore, the oldest, most skilful, and most expert amongst the Physicians, were at a loss how to treat it regularly and successfully.” Another says, “Upon the appearance of this new disease in Italy, a great many of the most eminent Physicians were at a stand, and refused to meddle with it; and with very good reason, for in curing diseases, the first intention, according to Galen, is to be taken from the nature of the disease, which, as there was no remembrance extant of this disease, it was therefore unknown.” Another complains “of the gross ignorance in the cure of this new disease.” Another says, “Lues Venerea was in the beginning very violent, hard to be cured, and fatal to many; because, as it was an unknown disease, and never before seen, the Physicians knew not how to treat it.” Another says, “For two years after the disease first appeared, the Physicians were silent about it; and they not only avoided touching such as were taken with this complaint, but shunned even the sight of them, which

they did not in any other disease." And another says, "At first the poor people who were infected with this distemper were expelled from human society, like a putrid carcase; and being forsaken by the Physicians, who would neither give their advice about them, nor visit them, they dwelled in the fields and woods."

An analogical application of mercury was not long neglected, and the application of it by the first Physicians confirmed the intentions. Such was the great success of one Practitioner, who made the first experiment of mercury (Berengarius of Carpi) that, according to Fallopius, he acquired a fortune by the practice of this secret alone. To him, and to John de Vigo, this eminent knowledge was chiefly confined. From their success, and from the candour of Vigo and Fallopius, mercury became the accepted and established antidote to the Venereal poison: and notwithstanding it has been subject to the grossest abuses, and has often fallen into temporary disrepute in consequence of a wrong and mischievous application of it, yet it has generally been approved as the only real and effectual antidote for extinguishing the venereal poison out of the constitution of a subject so infected. The original Practitioners applied mercury in the same form that it was advised by Rhazis for the cure of other cutaneous eruptions. They applied it in an ointment, and without knowing that its good effect was produced by absorption, they adopted the practice merely from consequences, as the loadstone is now in use by mariners.

In order to relieve the patient whose bones were afflicted by pains, they improved the idea of the effect of the ointment, and laid mercurial plasters upon the joints, and upon diseased bones. At this period, Chymistry had not enriched them with many variations of this wonderful medicine. But it should not be forgotten, that Vigo speaks of the *mercurius precipitatus ruber*, and recommends it as a dressing for chancrous sores. It is presumed also, that he ventured to give it internally, for it was not then unknown, that mercury, as a mineral, was poisonous to the constitution. Physicians who had used it as an ointment for the cure of cutaneous eruptions, had decried and forbidden the use of it internally, even at the time that it was first applied externally for the cure of venereal eruptions. So cautious were they in the application of this poison, that it was but a fortieth part at first, afterwards it was increased, so as to amount to a fourteenth part, and lastly to an eighth of an ointment. In this form it was found to be effectual for curing the disease. Together with their ointments and plasters, fumigations were in a very short time in practice; for as they judged that mercury produced a cure by coming in contact with the part alone affected, so was it necessary for them to contrive some mode of introducing mercury to the sores of the throat, and for that purpose fumigations were adopted.

The increasing success by mercury, and the wonderful influence which was displayed by the cure of

this important and dreadful disease, naturally gave an importance to its antidote. Chymists of that time engaged their attention to it, out of which many mercurial preparations came forth, and each laid in its claim for that preferable action and innocent security, which it possessed in a superior degree to former preparations, and former modes of administering the medicine. It was as early as the year 1553, that lotions of mercury sublimated were first in use, by Angerius Fererius. Two ounces of it were dissolved in six pounds of distilled water. He used to wash and to rub the whole body, excepting the head, the breast, the stomach, and the arm-pits; and he continued this for ten days, once, twice, or thrice in a day, according to the strength of the patient, and other circumstances. The patient, at the same time, was sweated most profusely; for an idea of sweating was also, like mercury, applied to the cure of Lues Venerea, and was first of all introduced from similar analogical conclusions. First, because it was cured with more facility in the West Indies; and secondly, because the end was obtained by the same means in the practice there, in aid of a cure by guaicum. Girdles of quicksilver for the loins, and bracelets of the same were worn upon the wrists. The art was strained to introduce mercury in a neat manner, which is a natural progress in all arts, either essentially or affectedly. But in this instance it was affectation alone, for an Indian would think his person adorned by that unction with which an

European is disgusted. The fumigations, which were applied, from the idea borrowed of their curing the Itch, consisted of mercury extinguished by turpentine; or cinnabar, mixed with some fat ingredients, that would easily take fire, and emit the most copious fume.

It appears that Vigo was the first of all the ancient Physicians who avowed giving mercury internally (for hitherto it was at least supposed to be confined to external purposes alone), and that was in the year 1535. The medicine which he gave was the mercurius precipitatus ruber, that preparation which had been praised by him, and more by Nicolas Massa, for its great success, when applied externally on venereal ulcers. The attempt of giving it internally was cautiously conducted, somewhat after the same manner some now anxiously wish, yet are afraid to give arsenic:—for Peter Andrew Matthiolus says, “a great deal of mischief may happen from taking this powder, unless it is first duly prepared.” This preparation consisted in washing it in distilled water, such as sorrel, and exposing it afterwards to the fire to dry. What would the Alchymist who invented the powder have said to this? I fancy he would have thought very inferiorly of the Physician’s chymical knowledge. The powder, thus prepared, was given in a quantity of five grains, and the violence of its action brought it into disuse. To the red precipitate succeeded pills with crude mercury, the receipt of which was brought from Turkey, and

they were called originally, pills of Barbarossa. These, according to the testimony of Peter de Bayro, Physician to Charles the Second, Duke of Savoy, in 1540, consisted of twenty-five drachms of quicksilver, ten drachms of rhubarb, three drachms of scammony, one drachm of musk, one drachm of amber, and two drachms of wheaten meal; the whole to be made into a mass, with lemon-juice. A pill of this of the size of a pea to be taken every night an hour before supper. Bayro relates, and I dare say with truth, that, these pills were wonderfully successful. But they were introduced at a time when the reputation of mercury was fading, owing to that general and improvident abuse of it, by its falling into the hands of ignorant Practitioners; for Bayro remarks, that (such was then the impression on men's minds about mercury) if any who had used this remedy were seized with a disease of whatever kind within ten years after, it was sure to be blamed for it.

Such was the misfortune, and such the fate of a great attempt in the improvement of the use of mercury, from its being brought forward when prejudice was deaf to its value, and would not permit its merits to be weighed by the standard of truth. The abundance of forms in which mercury is now given, is the consequence of pharmacy being more enriched by chymical inventions posterior to this time. Man may generally be defined (and history concurs in the establishment of the fact) to be fickle, improvident, and abusive. And the definition will more

strongly apply in this instance, than in any other which I am able to recollect. When Lues Venerea first appeared, such dejection and despondency prevailed amongst men, as to depress their minds to the lowest pitch of despair. But no sooner was the effect of mercury proclaimed, than its advantages were extolled as the only specific that could be depended upon.

The abuses of this specific, the flagrant evils arising out of the improvident use of it, the frequent instances of those who had fallen victims to its poisonous action, excited a general clamour against it, and many preferred enduring the disease, to the mercurial remedy. It is no wonder, therefore, that guaicum, when brought into Europe in the year 1517, China root in 1535, sarsaparilla about the same time, and sassafras a little after, were received with wonderful applause, as the deliverers of mankind. For they were received as capable of delivering the wretches who laboured under this disease, both from an untoward distemper, and a more untoward remedy. But it was soon found by unerring experience, that venereal cases were so virulent as to withstand all the efficacy of these exotic remedies. and to yield to regular salivation alone. From the virtues, therefore, of guaicum, China root, sarsaparilla and sassafras, being by repeated experiments farther ascertained, from their insufficiency becoming more notorious, and consequently from their falling every day more and more into disesteem, the Faculty

found it necessary, as no other effectual remedies were discovered, to try again what they could do with mercurial inunctions, which, if not wholly omitted, for some time had at least been very much neglected.

It is curious to remark, how the dates of the first introduction of these exotic woods into European practice tend to confirm the novelty of the disease, and tend to prove that inquisitive anxiety which engaged the minds of men in search of that remedy which would cure the disease by gentler means than mercury, and by means more conducive to the safety and comfort of patients. Guaicum was the first exotic imported, and was brought into use, from observing, that it was applied to the same purpose by those Indians who first imparted the disease to the European sailors. This was that exotic which they could soonest procure, after having experienced the disease, and this was brought home on the return of the third voyagers.

The disease having been propagated to India by the banishment of the Moors from Spain, they introduced China-root, and adopted it as a cure, which, in process of time, found a recommendation, in the like manner, owing to a general disgust to mercury. In consequence of the disease being propagated to the Spanish settlements in South America, the effects of sarsaparilla, a vine of that climate, were discovered, and that vine was brought over to Europe, and adopted from the success of it there. But whe-

ther from a difference which there may be in the woods being brought to Europe dry instead of green, or whether from a difference which climate will produce, on subjects afflicted with *Lues Venerea*, none of them were found to answer those high expectations which their virtues flattered. A similar instance to this has been lately experienced in hemlock.

As soon as it was generally known, that under the influence and predominate action of mercury, and under the various changes which the art of chymistry effected, could be given internally, then every country abounded with panacea for the cure of *Lues Venerea*. Swarms of secrets infested every town of every country in Europe, as if it were intended that by this disease society should experience additional scourges, from the abuse of that only medicine which may be justly said to be an antidote to it. From that time to the present, every secret which is avowed for the cure of *Lues Venerea* possesses for its basis one of those chymical preparations, not excepting such professed remedies which are extolled for their pretended power of curing *Lues Venerea* without mercury. *Mercurius dulcis*, the common *Æthiops*, and abundance of other *Æthiopic* minerals, according to the nature of the medicine joined to mercury, were introduced into practice. *Mercurius alkalizatus* ground with oyster shells; *mercurius antithisicus* with dry balsam of Peru; *mercurius antiscorbuticus* with gum guaiacum; *mercurius dulcis* with manna; *mercurius diureticus* with juniper gum: and *mercurius catharticus* with scammony.

But trials of these generally fell short of expectation, as the mercurial globules, which only adhered to the substances they were joined with, soon got loose in the stomach, and ran into larger globules, which passed through the prima via, without affecting the habit. These were the innocent and ineffectual inventions which followed the first internal application of mercury. To them succeeded many rough and pernicious tortures of this mineral. *Mercurius precipitatus albus*, solution of red precipitate in *aqua fortis* corrected, red precipitate, turbot mineral, green precipitate, besides many high-sounding panacea, such as to our ears are now-a-days familiar:—*Panacea solaris*, *lunaris*, *arcanum corallinum*, *aurum vitæ*, &c. Nor was an internal use of mercurial waters, in which sublimate was the basis, wanting to them. Eight, ten, or twelve drops of solution of corrosive sublimate, given in eight ounces of barley-water or water-gruel, was execrated as “the vile practice of London quacks,” by our countryman Wiseman, in the last century.

Such were the successive exhibitions of mercury, and such were the various forms adopted, when once it was found safe and practicable for being taken internally. That danger apprehended by Rhazis, and other Physicians, who confined themselves to the external use of this poisonous mineral in the genuine state of it, and which is the form most innocent to the constitution, being once, in the opinions of succeeding Practitioners overcome—that

fence, kept up through fear, being once broken down, the communication became open, and common to all. The great variety of impositions which were practised by this medicine, in former days, is equally applicable to the present. We have now pills, professed to be made without mercury, which salivate; we have now the disease cured without salivation, but the mouth is made abominably sore; and we have now our conserves and our vegetable syrups, which beguile the taste and other senses. But it is my part to inform, not to reform; I shall therefore spare my remarks.

The whole history of *Lues Venerea* demonstrates a constant rebellion in the inclinations of men for adopting mercury. From the frequent efforts that have been made to set the application of it aside, and from the abuse of it through error in practice, it has been in repeated danger of being decried. If there were within the scope of human invention any other certain remedy for the cure of this disease, I am inclined to believe that no sense of its value would be an inducement for retaining it a moment longer in practice.—When we decry the use of mercury, because of the baneful and poisonous effects resulting from it, we are only in fact arraigning our own conduct, and impeaching our own understanding and want of practical experience; for mercury cannot be considered otherwise than a powerful instrument put into our hands, for the purpose and application of which we are answerable. A know-

ledge of that power is to be acquired, and confidence is in course implied, that we adjust the application of it to the benefit of the patient, and that we watch and guard against that pernicious counterpart of the two-edged power which we know it possesses, and which we know is often laid to the disgrace of the medicine, when, if I were to speak out, properly is and ought to be the disgrace of him who so vilely administers it.

It is from a discrimination in the use and abuse of it, that we are to take credit to ourselves, for it is well known, and the idea is common, that mercury will cure Lues Venerea; if, therefore, there be no art necessary for obtaining that effect, if there be no art to be acquired for discriminating in practice, and if art does not consist in the mode of application of it, so as to have all the good, and as little as possible of the bad effects of it—if this be not the fact, then that which has been the contention of ages, has been about nothing; and that which is the contention of this day, is of as little importance. FOR AT THIS TIME THERE ARE SCHISMS IN OPINION, ABOUT THE APPLICATION OF MERCURY, AND OF WHICH I SHALL HEREAFTER TAKE NOTICE, WHEN I COME TO THE CURE OF THE DISEASE.

CHAPTER XII.

APPLICATION OF MERCURY FOR THE CURE
OF LUES VENEREA.

THE intricacy which is annexed to the proper application of mercury, so as to effect a cure of Lues Venerea by the safest and the surest means, is most strongly confirmed, by appealing to the difference in opinion, which has subsisted and does subsist to this day, about the most preferable and most certain method of administering mercury, in order to obtain that end which is aimed at by all, but in the completion of which the difference is wide. That method which is certain to cure, if one be more certain than another, is not so generally impressed upon the minds of men, as to become, by conviction, universally adopted. I do not say, that I am able to account for why there should be this difference in opinion about that which is the most preferable method of administering mercury, because I cannot in truth reconcile myself to a belief, that there ought or can be a difference in opinion about the administration of it, without one of the opinions being at least egregiously wrong.

It appears to me, that very often the difference in opinion about the administration of mercury, arises

from that antipathy which Theorists have conceived by now and then seeing the baneful effects of mercury, in cases where it has been improperly gone on with. The power of mercury must be always the same, and if every constitution to which that power was applied were the same, then it would be possible to lay down a rule, prescribing the exact proportion of it which would be necessary to procure the safest and the most effectual cure. If that were the fact, there would be a given rule, to direct the due administration of the medicine, so as to procure, to a certainty, the desired good effects from it. But since every subject possesses his own proper idiosyncrasy, since every subject is exposed to varieties of changes from varieties of diseased conditions, since the venereal symptoms will be more or less rapid, malignant, and obstinate on one subject than on another—it cannot from these causes be expected, that an exact proportion of mercury will ever be ascertained, which, by curing one subject, will thereby be the standard in truth of that proportion which may be necessary for curing another. This is not to be done through any given quantity of mercury. It is enough to know, before the cure of the disease on any constitution be began upon, that there is a power in mercury adequate to that end: as for the rest, experience must be the guide. It is a knowledge of the action of mercury upon the constitution in general, a knowledge of the action of it upon a constitution in particular, it is a knowledge

obtained from a variety of cases, which will alone teach how to regulate the administration of mercury to the best advantage. The true line of practice must be a result of observation and experience.

It will be therefore of some importance to reflect, what those Authors have said upon the subject, who have been most in the way of ascertaining this knowledge; for it is to them I should look up for an opinion upon the question, if I wanted to have my doubts satisfied when they were wavering; it is to them I would rather appeal for the truth, than to a Theorist, who, by seeing that mercury is capable of injuring the constitution, thinks the proportion given of it is, from that cause, always too great; and who keeps alive a controversy, by laying down such rules and distinctions, which those who are more conversant in practice must know, from their experience, cannot nor will not answer; and which those who are conversant in practice must know, that observations like these are the result of theory alone, and not of practice. It is from conclusions drawn in the closet, persuasions have gone forth to the world, that the use of mercury is abused, by being too profusely administered. And it is from want of an enlarged knowledge of this subject, that such false conclusions have been drawn.

It is necessary for mercury to be pressed forward to a certain point: it is to be wished, that the point may be obtained without any bad constitutional effects. It is necessary that the venereal symptoms

be extinguished, and for that end, that certain effects, arising out of the operation of mercury on the constitution, must come forward: it is to be wished, that as soon as these effects do come forward, the venereal symptoms may retire, and that the constitution be left with this specific difference, in consequence of the exchange of a mineral poison for a morbid one—that the mineral effect will cease, when the application of it be withdrawn from the constitution; but that the morbid effect will never cease, until the operation of mercury has gone on to a certain acmé, and until it has brought about this great and necessary revolution.

The Theorist always presumes that mercury can cure, in small quantities, Lues Venerea, because he has seen instances where the patient has got well, who has gone abroad in the air all the time: and he presumes that large quantities of mercury do not cure, because he has seen instances where the effects from mercury have brought the patient into a worse condition than he appeared to be in from those venereal symptoms which the mercury was appointed to cure. These are the two leading arguments, which every Theorist has furnished himself with, from the beginning of the application of mercury for the cure of the disease, to the present time. Every objection which has been urged against that free use of mercury which is adopted by the most experienced Practitioners, is founded either upon the possibility of the patient getting free of the disease with less mer-

cury than is used, or upon the mischief which is produced on the constitution, in consequence of an improper freedom in the use of it.

Such presumptions, so far from their being founded in envy, are founded in philanthropy ; and so far from their being founded in truth, are founded in error, for want of knowing the truth. The Theorist and Practitioner are urged on from the same purity of motive, to express their opinions. But the difference consists in the advantages which the Practitioner has over the Theorist, and which tend to render his presumptions abortive : for the Practitioner finds that, unless the virus be extirpated, the infection will be again re-inforced, and again return, with aggravated symptoms : and the Practitioner is taught from experience, that it is absolutely necessary that some outward visible signs of the action of mercury be apparent, before any of its known power over the symptoms of *Lues Venerea* will be demonstrable ; and that therefore he cannot reconcile his expectations to that dose of mercury which is to be found in a formula, to that exact proportion which may be allotted to every subject. He must, to be successful, press forward mercury, until its efficacy be apparent. Nor must he be regulated by any other rule, than by the consequent effects which the mercury produces.

Every young man, when he begins practice, possesses an idea, that the disease can be effectually cured with a less quantity of mercury than he finds

will be necessary, as he proceeds in his practice; and almost every practical instance will convince him of the necessity, from the nature of the case, of changing his opinion. He must, if he means to be successful, not consider the weight of mercury that he applies, but consider its effect, from an application of it, upon the symptoms and upon the constitution. I do not vouch for the truth of this position from the result of my own observations in practice alone, but I appeal, for the confirmation of it, to every practical Author, from the first use of mercury, to this time: I appeal to that opinion which has uniformly been given—to that opinion which Authors were obliged to give from necessity; because, if their practice had not been to press forward mercury, until the venereal symptoms were completely eliminated, they could not have been said to have been successful Practitioners; and if they had not given to mercury such a scope for action, they could not, by it alone, have cured the disease. They could neither have maintained their high characters as Practitioners, nor their assumptions in behalf of the omnipotent power of mercury over Lues Venerea.

The Theorist who condemns a liberal use of mercury, and who urges the infortunia which arise in consequence of it, as his justification for thus condemning it, is silent about the consequences which must inevitably follow, from Lues Venerea being left to continue its baneful progress upon the consti-

tution. He does not recollect, that if mercury be necessary to be fresh applied, more rather than less must be required, in proportion as the disease has continued its action—he is silent in comparing those transitory effects which are produced by mercury with those permanent effects which are produced by *Lues Venerea*—and he is silent about the necessity of the disease being cured at all events, and that for effecting it, mercury must be pursued until the end be obtained, because no other known power besides can procure it. This happens to be one of those permanent truths, which neither ancient nor modern refinement has ever been able to do away.

The respectable Authors who have urged the necessity of pressing forward with mercury until a change, in the constitution be apparent through it, will be seen to add weight to the arguments which I have advanced. The names of Vigo, Massa, Fallopius, Berengarius Carpus, Sydenham, Astruc, Turner, Howard, all concur, *unâ voce*, in support of this opinion. Astruc has given cases where the disease was left behind, and where the patients would have been utterly lost by it, if they had not fallen into his hands, and if he had not gone on with mercury.

One was the case of a young nobleman who was infected with *Lues Venerea* in Holland, and after repeated unskilful treatment, he went to consult Doctor Boerhaave. At that time he had erratic pains in his joints, and painful exostoses on the

upper and lower parts of the right arm ; but his chief complaint was a putrid, malignant ulcer in the upper part of the nose, with a caries of several appendages of the œthmoidal bone, of which several small fragments had come away. Dr. Boerhaave, according to the method which he proposes in his preface to his third edition of his *Aphrodisiacus*, prescribed for him a decoction of guaicum, to be drank according to the strictest rules of diet. The complying patient pursued that method with the greatest exactness for three months, till being quite emaciated, squalid, and withered, he was hardly able to stand, or even to draw his breath, nor in a condition even to suffer that remedy any longer. However, by that method the pains left him, the exostoses diminished, his nostrils grew less fœtid, and discharged pus in a smaller quantity : wherefore, having considered the case, Dr. Boerhaave judged the patient to be perfectly cured, and ordered him to return by degrees to his usual manner of living. Three months after the patient came to Paris, and applied to Dr. Astruc, who found the exostoses remaining of the arm, and which were painful to the touch ; the discharge from his nostrils was offensive, a good deal of pus mixed with mucus came from them ; his nose was red, swelled, and painful to the slightest touch ; caries bony fragments were often discharged ; the septum, being gradually wasted, came away. A continuance of inunction for three months induced these parts to heal, dissolved the exostoses, and the patient was restored.

Thus the variance of the theory of Boerhaave and the practice of Astruc were at issue; and as it will ever be the case, so it was in this, perseverance in mercury prevailed: it was experience alone which could have induced Dr. Astruc to proceed with mercury. From the like experience we have ascertained, what reason would never have suggested, those different effects which are produced through mercury, whether it be taken inwardly in chymical preparations, or mixed with lard and externally applied.—We have ascertained also, that, under necessary regimen, Lues Venerea, in process of time, is certain to yield to the action of mercury—to the predominant power of that mineral which possesses neither taste nor smell.

Sydenham, one of the first and most sagacious observers of the animal œconomy, not only watched with accuracy the symptoms of the diseases, but also the symptoms which remedies produced. He does not hesitate to say, that he believed Lues Venerea was not curable by any quality in mercury, purely specific, but by salivation alone. When he said so, he was not very wide of the mark: for Sydenham reasoned after this manner, before he came to so declaratory a conclusion:—If mercury be given in any quantities, and continued for any length of time, I do not see that mercury, as mercury, cures Lues Venerea: but when I see from the consequence of mercury, that the salivary glands become affected, I then, upon observing the vene-

real symptoms, find that mercury, not as mercury, is curing Lues Venerea, but that mercury, as affecting the salivary glands, is curing it.—So far Sydenham.

And I say that there must be a necessary mercurial disease produced on the constitution by mercury, before Lues Venerea can be affected by it. There are two of the strongest reasons for this conclusion to be given, that can possibly be adduced; the one is the result of practice, and the other is founded in necessity. Can it be thought that our ancestors, from observing that mercury, when it did act upon the constitution, produced strong symptoms of a poisonous nature, and which, when carried to excess, would certainly poison and thereby destroy the patient—I say, can it be thought that they would, in the face of such effects, have persisted in pursuing the same system of applying mercury for more than two centuries, that they would have persisted in avowing that these symptoms must come forth before mercury could be fairly proclaimed triumphant over the disease? Can it be thought that they, as well as we, would have persevered in looking after the symptoms which are produced by mercury, instead of looking after the quantity, if that quantity availed any thing in the cure, without mercurial effect?—Certainly not. Experience told them, that it was effect and not specific mercury: and that which experience told to them and to Sydenham, it has alike imparted to us. There is a

necessity for the effect to be produced, or any curative expectations from a specific power in mercury will be hopeless and deceitful.

DEFINITION OF THE SYMPTOMS OF MERCURY ON THE CONSTITUTION.

I shall relate what I understand to be the positive and the presumptive effects of mercury upon the constitution. The positive effects are, an acceleration of the pulse, an universal prostration of strength, a giddiness of the head with swooning, a bleeding at the nose, an increase of saliva, which will be viscid, and attended by pain and swelling of the maxillary glands, a brassy taste in the saliva, a stubborn costive habit, an aching head, a throbbing in the temporal arteries, and the eyes hot and red-dened. If these symptoms be supported by repeated additions of mercury, the affections of the mouth will be increased, the discharge of saliva will be more in quantity, the gums will become sore, will bleed and ulcerate, the teeth will be loosened, and the insides of the cheeks will be furrowed by ulcers forming on the sides of the teeth. Ulcers will also be formed behind the teeth, which will give pain, and often prevent the mouth from being opened. The tongue will swell, ulcerate, and fill up the whole of the mouth—parts of the tongue and parts of the cheeks which come into contact

with the teeth, will particularly be ulcerated. By additional mercury these symptoms may proceed to a dangerous excess. The tongue will protrude out of the mouth, without there being a possibility of returning it, and it will be indented by the teeth. The viscid saliva will perpetually flow. The patient will be incapable of taking any nutrition, and of expressing his hard condition by speech. His stools and his urine will be bloody. His pain will be exquisite, no sleep will come over him, the whole of his face will be greatly swelled, and he will become impatient, irascible, and frequently furious. If mercury were farther pursued, and if such a condition in the patient were in the least aggravated, the consequences might then be fatal.

Such are some of the positive symptoms which arise from the action of the poison, and such are the effects which a profusion of mercury will ordinarily produce upon the constitution. But it sometimes happens, that by any quantity of mercury these effects cannot be produced. To be able to produce them, it is always to be wished; a certainty of producing them is not always to be obtained. When mercury has been pushed to a certain point, if the positive symptoms of salivary affections do not follow, there are generally some extraordinary symptoms which will be discovered, and which may be presumed to be alike declaratory of the action of mercury upon the whole of the constitution. These are, foetid breath, prostration of strength, throbbing

of the temporal arteries, inflammation of the eyes, profuse perspiration, diarrhœa, bloody urine, and swooning. Whilst perspiration or diarrhœa continue, salivary affections will not come on: but the mercurial diarrhœa will be attended with as much serious importance, when carried to excess, as those salivary affections which I have been describing; and what is still worse, the venereal symptoms will not so readily yield to the action of mercury falling on this channel, as to the action of it falling on the salivary glands.

Independent of all these effects from mercury, both positive and presumptive, there are presumptive indications of the action of mercury, which seem to arise out of a concurrence of effects, rather than out of a simple effect of mercury. It has been presumed, when mercury has been administered in large proportions, and when the patient has gone forth in all weather, and has eat and drank freely in society, that from these causes, mercury has brought on an ungovernable frenzy, and which has either continued without intermission, or has subsided, leaving the patient with a perfect or imperfect restoration of faculty. This is a presumptive effect which has been attributed to mercury. I know no reason why such a suspicion may not be presumed.—Seeing that mercury is arbitrary in fixing upon parts, and seeing that when perspiration is checked, and the blood is in an irritable state from excess in drinking, I think it might be concluded, that such

an effect is possible. Other effects from mercury are, cholic, attended with paralysis of a limb, or general paralysis. Such have been known to follow an excess of mercury, but have not been known as incipient symptoms of the poison, but rather as symptoms in consequence of the poison and of indiscretion together.

I have thus briefly stated the effects of mercury upon the constitution, when carried on to any excess. I do not mean to point out, that these effects are absolutely necessary to be present for a cure of the worst symptoms of Lues Venerea. I do not think they are: but it is necessary always, that some, if not all of them, should discover a disposition for following. It is necessary that the constitution should be in a state preparatory for their coming on, before the action from mercury will be sufficiently capable of eradicating venereal virus. It is necessary that there should be a putrescent diathesis, foetid breath, prostration of strength, affection of the gums, and increase of saliva; and that these appearances should be supported for a considerable time, even longer than the venereal symptoms remain, in order to eradicate the disease from the constitution. This may be almost said to be that desirable point of mercurial operation upon the constitution, which is calculated to effect every purpose of eradicating venereal symptoms. Without these mercurial appearances, without these positive inferences, that the whole of the fluids have under-

gone this necessary mercurial change, and have been continued, for some time, by a regular succession of mercurial supply, I should not expect that a fixed, obstinate, and inveterate venereal infection could be eradicated.

This mercurial change which is brought on the constitution, is clearly of a putrescent nature. The whole of the symptoms concur in proving it to be so. The crasis of the blood is attenuated. The smallest vessels of both the arterial and lymphatic system are strongly affected by the mercurial action. The eyes look fiery, the pulse is quickened without being high, the chamber of the patient is strong of a putrid stench, the urine is fœtid, and of a deep dirty red, the body is either costive, or colliquative, voluntary hæmorrhages flow from the nose, and these dispositions are always increased by an exclusion of fresh air. The prostration of strength, the fretfulness of the temper, the dulness of the senses, and the inactivity of the mind, are declaratory of such a disposition from the action of mercury. And this is confirmed by reversing the scene — for, as these symptoms are only supported by a constant supply of mercury to the constitution, so whenever that supply be withdrawn, it is wonderful to see how the effects decline, especially if the patient change his situation to pure air, cleanse his body by a tepid bath, take that salutary restorative nitric acid, and gentle exercise in the open air: in the course of

three weeks there will not remain one solitary trace of mercurial symptoms.

It must be evident, upon reflection, from what I have said, that that which is deemed to be the most objectionable point in the action of mercury, namely, the excess and violence of its action upon the salivary glands and mouth, is, of all other points, the most devoutly to be wished. To be able to procure this point, is the desideratum of every Practitioner: for by it he will feed the constitution with such a supply of mercury as will always produce a certain, regular, and safe effect — he will be enabled, after having brought its action forward to a certain point, to continue it there,— he will be enabled to obtain a cure of the disease, and a commanding controul over the unnecessary action of its antidote.

But if, on the contrary, the disposition of the constitution be not favourable to the production of these symptoms, if that necessary change cannot be brought about, and cannot be made known through the positive effects of mercury, there is every reason to presume, that the obstinacy in eradicating the venereal virus will be exactly in proportion to the obstinacy of the constitution in resisting this necessary mercurial change. Therefore, I shall take upon me to say, that the most favourable circumstances we can hope to attend a cure of the constitutional Lues Venerea, are, when a fair proportion of mercury, by being gradually absorbed into the habit, as gradually excites a salivary action — when, by continuing a

supply of mercury, this effect is uniformly supported for some time after the venereal symptoms have totally disappeared—and when, by withdrawing the mercurial supply, the symptoms of it as gradually decline, leaving the patient freed from both the Venereal and mercurial disease, and the constitution in every respect convalescent.

This is the desideratum that, in every case, we should have in our view—and the more every case is found to deviate from this, the more untoward it will prove, the more the patient will be harassed with the disease, and the more will the baneful effects of the remedy prey upon him. If mercury does not cure the disease, when it is applied for the purpose of curing it, the constitution of course will be exposed to endure a double violence, a double excess of poisons.

Notwithstanding it has been the fashion to decry confinement during a serious application of mercury for the cure of an inveterate venereal constitutional infection—confinement must be adhered to, it must be so:—and whoever gives up this point in practice, whoever makes a surrender of it, in compliance with the opinion of any man, whether patient or not, will have cause to repent. It will not prove so prudent or safe to attempt that which is impracticable, and return afterwards to that which is practicable, instead of beginning with that which is practicable. Whatever be done against the conviction of our own internal evidence of things, is not honestly done.

There is a necessary confidence arising out of conviction, which never should be surrendered. If there be a truth which I am more than ordinarily interested in, I am in this—that it is much more easy to the patient, and much more safe—that less mercury will be required to effect a certain cure of Lues Venerea, by confining him to his chamber, than can be dispensed with when he goes at large, putting the safety and the certainty of his cure out of the question, for that must always, from wrong treatment, be doubtful and hazardous.

When the constitution is evidently under the influence of venereal virus, that influence not only extends to such parts as betray the immediate and apparent symptoms of venereal action, but at the same time the whole of the solids, the whole of the circulating fluids, and the whole of the secreted fluids, are also possessed of its influence. The great and predominant power of mercury consists in that quality which belongs to it as a poison, being capable of pervading every part of the constitution where the venereal virus can pervade, and of overcoming and radically destroying it. If, therefore, it be necessary that these evident effects which arise out of the operation of mercury upon the constitution, must be produced before the constitutional influence of it can overcome the venereal influence, if this decisive mercurial change in the whole of the component parts of the constitution must take place before a cure of Lues Venerea can be effected, it is not only

a professional duty, but it is exacted from us to pursue the safest and most effectual means for obtaining that end.

Notwithstanding the plea for permitting the patient to be at large, under a course of mercury for the cure of *Lues Venerea*, be plausible and pleasing, yet it must be obvious that it is a false one. Besides, there is an indelicacy, which is the smallest of all objections, in a patient obtruding himself upon company with a foetid stench emitted upon every expiration, and with a countenance every thing but agreeable.

I shall proceed to consider what these auxiliary aids are, which may be deemed necessary and consistent for promoting a salutary operation in a mercurial process, and for conducting the patient in the safest and most effectual manner during a cure of *Lues Venerea*—Confinement to the chamber, a regular diet, a gradual introduction of mercury by friction, an exclusion of fresh air, are the most important points to be attended to. The frictions must be begun in moderate quantities, until symptoms of mercurial action, from a repetition of these frictions, be discovered : and when these symptoms are of the positive description, the frictions must be so repeated as neither to decline nor advance in too rapid a proportion. When the symptoms advance too rapidly, nothing is gained, but much must be lost, from the regularity of the process being interrupted ; for the cure consists in persevering with a

regular supply of mercury, until the venereal symptoms be extinguished : and there will be often some venereal symptoms which will not nor cannot be brought to yield under a process short of two months : then it must be easily understood, that, if the symptoms arising from mercury be so alarming as to forbid a perseverance in it within the first fortnight, the process will thereby be considerably checked, time will be lost, and the case will be rendered more uncertain. Therefore this must be attended to.* It must also be observed, that on the other hand, the mercurial supply must be constant and uniform : for, as soon as it be discovered what proportion of that supply is necessary for keeping up an equality of effect, for supporting that uniform ptialism, which will be necessary to be continued for some time after the symptoms of a long prevailing venereal infection have disappeared—then the difficulty as to the future regulation of the proportion will cease, and the process will be conducted with as much facility and security as the nature of the case will admit. Mercury, therefore, must not be hurried into the constitution ; the action of it will be various upon various constitutions : the business of the Practitioner is to ascertain, from consequences, the particular power of mercury upon every particular constitution, and to repeat and regulate the supply of mercury accordingly.

The beneficial effects which are produced by the action of mercury, when that action is kindly, must

always be the same upon every constitution : but the proportionate quantity of mercury which will be found requisite for producing such effects will be various upon various constitution . From the advantages obtained by confinement to the chamber, and by exclusion of the external air as much as possible, the cure will be conducted within less time, with less mercury, and with greater certainty : the natural action of mercury will meet with fewer interruptions, as the diet will be regulated, and the putrescent diatheses promoted and confounded. As the whole of this process consists in a combination of the most favourable means for rendering the operation of mercury, for the cure of Lues Venerea, speedy, safe, and effectual, so will any practice, adopted in opposition to this process, increase the difficulty and the danger. The more the system which I have insisted upon be deviated from, the more will obstacles arise to the cure. I mean to say, that it is impossible for a patient to be cured of an inveterate constitutional venereal infection, when he is, during the attempt, going into the open air; and it is dangerous even to attempt it. The necessary mercurial change in the constitution will be constantly resisted, and more mercury will be required from that cause. The longer time this necessary change be producing, the more uncertain it is of being produced. Longer time will be necessary for continuing the mercurial supply; that supply will be more interrupted in its action, and the prospect cure will be more uncertain and distant.

In order to effect a radical cure of the most stubborn venereal symptoms, I do say, that the change which is wrought through mercury, must be complete and decisive: and I do say, that this change cannot be generally brought about without confinement, and without every accelerating power being employed to promote and conduct the mercurial influence. If these arguments were strictly adhered to, and if the practice of them were always carried into effect, I am confident that the histories which we daily hear of cases — such as *Lues Venerea* returning after repeated salivations, and such as are calculated to degrade the action of mercury, both as a baneful poison and an ineffectual remedy, would be much more rare than they now are found to be.

From the opinion which I have given, that the strictest attention must be engaged to controul and regulate the mercurial action — it will be obvious, that any hopes or any expectations, founded in reason, of these events being either produced, or guarded against if they could be produced, must be fallacious, by the pursuit of any other method different to that which I have exacted. And although there may be still a difference in opinion, how these effects by mercury are produced, and how that change is brought about by mercurial action, which I have attempted to account for, yet the adoption of this prudent and natural system of curing the disease is neither new nor strange: it is that system which the most experienced appeal to in the first instance, and

it is that which the most experienced must adopt, after having tried other systems (or rather no system) before the intention will be completed. Why do those appeal to this resource in the end, if, without confinement, Lues Venerea can be cured by mercury? Why should this be considered as the last resource, by those who have given the preference, in former trials, to its opposite treatment? If all these changes could be effected by an open treatment, that would long ago have banished this out of practice. If every fluid in the constitution could have been so mercurially possessed as to defeat the venereal infection by this open treatment, the possibility would have been before now ascertained. Or, if an irregular salivation, conducted either by an open method of treatment, or a confined one, were sufficiently effectual for the intended purpose of eradicating Lues Venerea, there would have been no appeal for new trials of mercury, no repetitions of the remedy, no changes introduced in the treatment, importing a more serious and attentive concern to the action of the remedy, to the time of continuing it, and to the closer confinement of the patient.

There has been, of late years, an alterative method recommended for the cure of Lues Venerea, when the constitution is infected. This alterative method consists in giving such small doses of mercury, accompanied with sarsaparilla, as two grains of calomel, or five grains of quicksilver in a pill, every day, or at most twice a day, so that none of

the emunctories shall be affected through it. This alterative method is that which is adopted for local infection ; and, even in local infections, it is sometimes known to fail. It sometimes leaves Lues Venerea behind it, owing to the resistance which is given to the action of mercury, by the patient being in open air, and by irregularities which he may be frequently tempted to give into, through the company with which he associates. For to give this method the best praise, when it does succeed in local cases, it does not so well succeed as a less quantity of mercury would, under a stricter regimen ; and that stricter regimen (by which I mean to include confinement, although not with all its concomitant rigidities) which I have exacted for the cure of a constitutional venereal infection, is sometimes found necessary to be adopted even for the cure of desperate Chancres.

At any rate, most truly venereal Chancres will require an internal use of mercury for the cure of them—at any rate, that mercury, in order to be of any effect, must have had an influence on the part by its internal action, before most Chancres betray any change, by ceasing to spread, or commencing to heal. Then it must follow, and there cannot be a doubt of the fact, that, in this instance, the whole of the constitution must have undergone an incipient influence from mercurial action, when a particular part of that constitution evinces an evident proof of that action. And therefore I may venture to declare

it as my opinion, that the mouth should be somewhat mercurially affected, and that affection continued for some time after a venereal Chancre has healed.

There is a prevailing practice, during mercurial frictions, of making use of the warm bath, in the course of a cure under a strict regimen. I do not think that the practice can be justified by rational theory. This was borrowed from the French Surgeons, and I know they indiscriminately persist in it. But so far from its promoting the intention of a proper action of mercury for a cure of the disease it is appointed to cure, it tends to baulk it: for the effect from it is to open the pores and thereby to divert the action of mercury from its natural and proper channel. It hinders the progress of mercury in proceeding to accomplish that change which must take place in the *VASCULA MINIMA*, and disappoints the Practitioner from drawing true conclusions of the particular action of mercury upon each particular constitution: a point which I have said is always important towards conducting the cure. But if a salivation has been found to exceed those bounds which may be judged to be safe and to be necessary, if from any cause an abatement of that immoderate flux be the object—in that instance both the warm bath, as well as opening medicines, ought to be in practice for obtaining that end. A costive habit, from mercury, will be often so very stubborn, that almost a double purgative potion will be necessary. The

warm bath, nitric acid *, and opening medicines, are essential means for reducing exacerbad symptoms from mercury.

Sulphur has been named, as having a specific power of neutralizing quicksilver: but that argument, *a priori*, is foreign to any reason which is annexed to the action of mercury upon the constitution. It is improbable upon the very face of it: nor does the administration of it strengthen the idea by its effects, for I have never seen any good or any harm in it. Keeping the mouth clean with barley-water and honey, in the first place, and, when the sores will permit it, adding thereunto tincture of myrrh, will avail something. Nitric Acid, at any stage of these symptoms, will accelerate the cure of them: but the constitution, after a mercurial process that has been well conducted, will stand in very little need of any other restoratives than free air and wholesome diet; for though a kindly patient, immediately after a mercurial course, will be thin, yet his spirits will be high, his countenance will be cheerful, his skin will be clear, his constitution will feel relieved from a burden, and these will be expressed in every sense.

The action of quicksilver upon the constitution cannot be that which Dr. Astruc has attributed to it—it can neither be owing to the specific gravity of

* Practitioners in England and Europe are indebted to Dr. Scott for his report of this valuable medicine, the result of his practice in the East Indies. It can be given when mercury cannot, and afterwards mercury can be given with success.

it, nor to the power which there is in the mineral of being so minutely divided into globules, as to be enabled, by these subdivisions of globules, to pass through vessels which are too minute for admitting blood, and by that means remove obstructions impervious to any other medicine whatever. I say, that the experienced action of quicksilver is not produced by either the one or the other of these causes, and I prove my assertion by strong circumstances in the action of quicksilver, or any chymical preparation of it. That it is neither owing to its specific gravity, nor to the subdivision of its globules, is proved by observing, that sometimes, from a chymical preparation of it, a salivation can be excited, and that sometimes from a very small portion of it. I have seen six grains of calomel swell the face, make the mouth sore, and excite a spitting. It is the action of mercury even to the Minima Vascula which produces the effect of extinguishing venereal action. The action of mercury would never be considered as a relief to the constitution, by destroying the action of venereal virus, provided mercury, when it were once admitted into the habit, had a power of supporting its action, independent of additional supplies of mercury. If the action of mercury were not of stated duration, if it were a continued action, like the venereal, even though it did destroy the venereal action, I do not believe that any one would be induced to make an exchange of the latter for the former: for of the two, and whilst it continue, the

stimulus excited through the mineral poison is far more excessive than that excited by the animal poison. It is not only by the perception in our feelings that this superiority is proved, but also by the stimulus of mercury being superior to the venereal stimulus, and thereby destroying and extinguishing it.

The action of mineral poisons upon the human constitution cannot be supported without constant supply, but the action of animal poisons can. The action of mineral poisons is acute and almost immediate. Mercury, once imparted to the constitution, will not be capable of supporting its action beyond a certain duration; but venereal virus, once imparted to the constitution, endures for ever. The action of mineral poisons is of a certain duration. The action of venereal poison is supported by the original poison being multiplied from an atom of venereal fluid.

The most preferable mode for the application of mercury I consider always to be by inunction: I consider it the most innocent and the most certain action. The violence which it commits upon the constitution in general, is the only violence which is experienced by it from this mode of introduction. No violence is done to any local part. But when mercury is given internally, there is a greater probability of a double violence being incurred through its action: a local violence, besides the constitutional violence. It will nauseate upon the stomach, it will

stimulate the bowels, it will injure both, it will produce griping, and the dose will be obliged to be limited. In desperate cases, or when from any other cause the proportion of mercury is necessary to be increased, that cannot be so well and so safely done by internal application of any chymical preparation of it, as by external frictions. But notwithstanding generally there may be no hesitation about the preference being given to the external application of mercury, yet an internal application has its use, and may be sometimes employed to great advantage, as it will appear from the truth of the observation that I am about to offer.

In impoverished habits, we know that absorbents are sometimes rendered incapable of doing their office, in consequence of a long and symptomatic fever, such as is supported by the venereal stimulus. It may, without a stretch of reason, be presumed that their power may be impaired, if not totally suspended from that cause; and more especially, if the patient who is venereally infected be also in the constant habit of drinking spirituous liquors. These, I think, are causes why sometimes the largest proportion of mercury, applied in unction upon the surface, does not procure an apparent effect in any degree to the quantity applied. In such cases, and in such conditions, I have found this difficulty in the action of mercury most to prevail. It appears to me, that little out of the much which has been administered has been absorbed, and that little which

has been absorbed, has not been equal to excite a mercurial stimulus, much less, therefore, has it been equal to excite a stimulus superior to the action of venereal stimulus. When this is the condition of the habit, the most remote absorbents, which are those that are dispersed upon the cutaneous surface, will of course, by being remote and more minute, feel the effect of such a temporary disability, of such a privation of action, in a greater degree than the larger absorbents — than those which are nearer to the central part of the animal machine: for, if the absorbents throughout the *prima via* were also thus equally debilitated, the constitution could not then be supported. In such instances, therefore, the internal application of mercury, either partially or wholly, has a preference. In scrophulous habits, it is more prudent to try a solution of sublimate mercury in decoction of sarsaparilla.

Whether it be prudent to attempt a continuance of mercury, in any form, in a constitution thus conditioned, and when such is the result of its application, that must depend upon the necessity and pressure of the occasion. If the symptoms of venereal virus be rapid, either locally or constitutionally — if there be no time to be lost — if there be an immediate necessity to put a stop to them, an endeavour must certainly be made, by every possible means, to excite a complete mercurial stimulus, as the only means for effecting that end. But I believe that, in many instances, the effort will be vain,

and the end cannot be obtained: and I know, and I feel that such conditions are the most nice, dangerous, and intricate, of any which the disease and the remedy are exposed to. When mercury can be dispensed with for a time, it certainly ought to be; and pure air, milk diet, with light nutritious food and gentle exercise, must be the intervening resources. Nitric acid, bark, and steel, will be found to be the truest medicines for the purpose.

The chief causes of the constitution being brought into such a state, are, constant use of mercury, in small and ineffectual doses, in conjunction with hard drinking during the time of thus administering it: an ineffectual stimulus is thereby excited, which provokes, but never produces any change upon the venereal symptoms. No power which mercury can impart, can be received with radical effect, because the system is rendered incapable from debility, and because wanton and injudicious stimuli have been, for a length of time, mischievously and irrationally excited.

In all cases, therefore, it is of the utmost importance, when the administration of mercury is begun with for the cure of Lues Venerea, to observe temperance: it should be scrupulously exacted: and in all cases where mercury is once begun upon, it should be regularly pursued, until such a change in the constitution through it be produced, as proclaims it to be equal to the purposes of extinguishing venereal symptoms, either locally or constitutionally.

The dose begun with should be the smallest, and that should be daily increased every rubbing, until the fœtid breath, the prostration of strength, and soreness of the mouth, demonstrate its action on the fluids. The mercury should be followed up until the venereal symptoms have disappeared, and all thickening of the parts and induration have subsided. But if, on the contrary, the doses be not repeated daily, if they be not increased, if there be lapses of time between their repetition, if the stimulus be partial, but not effective, the constitution is thereby harassed, but not relieved: it has to endure a continued non-effective stimulus from mercury and venereal stimulus at one and the same time.

It is to be remembered, that the generality of those whose venereal cases are complicated with bad conditioned habits, provoke their own condition by intemperance and turbulent resistance to salutary advice: and that those who are not disposed to do well for themselves, will not shew the least inclination to do justice to Practitioners. We are frequently condemned by such, for what human foresight could not have prevented. But if we make the praise or blame of others the rule of our conduct, as a great Author has expressed it, we shall be distracted by a boundless variety of irreconcilable judgments, be held in perpetual suspense between contrary impulses, and consult for ever, without determination. Let us think for ourselves.

CHAPTER XIII.

ON THE CURE OF CHANCRE.

I shall begin with the most simple of all venereal Chancres, then by degrees go on to those which, from longer continuance, and upon worse habits, were not from the beginning so simple from that cause, and which will not in their progress assume any other more favourable appearances than those they first sat out with.

TREATMENT OF A SIMPLE SORE ON THE PENIS.

When the substance of the Penis is pallid and dry, a Chancre will not be so disposed to spread and inflame the parts around, as when it is red and moist. The cure of every venereal Chancre, be it ever so mild and simple, will generally exact the internal action of mercury. If a patient apply the very instant he discovers a Chancre, and if that Chancre be a little pustule, which just begins to discharge, which shines as if a pure fluid were lodged upon it, and which has not eaten any depth, and even before there be any apthous bottom to it, can or ought that Chancre to be cured without mercury

given internally? Admitting that there is a doubt, which the most accurate judgment cannot solve, whether the virus has been absorbed, or whether it be confined to that little point where inflammation and destruction of the cuticle has just marked out the seat of an incipient Chancre — what in this instance is the safest and surest method for securing the constitution from the possibility of future infection, and for inducing the Chancre, which is the part on which the foreign virus is acting, to heal kindly from its natural disposition, it being no longer stimulated or acted upon? Can this end be obtained by any topical application which is now known and applied in practice, and independent of the internal action of mercury? There are certain topical applications which may sometimes so succeed; but that is not such a success as I would wish to prescribe or to practise, because of the insecurity and uncertainty of it. Do those who favour the application of topical remedies trust to them? Or do they at the same time annex for a better security an internal use of A LITTLE MERCURY? For what purpose, when there is no inducement from an irritable inflammation on the parts, are topical applications — what is the motive for which they are applied, and of what do they consist? Is it fitting that any of them should be put into practice in this simple stage of the case — or will they answer fully the intention?

FIRST, OF EXCISION OF THE CHANCRE.

Where excision is practicable, it must be done immediately upon the first discovery of the Chancre, and the situation of the Chancre must be favourable for the operation, for there are many parts exposed to Chancres, where it would not be practicable; not to mention, that often there will be many Chancres on different parts. If a Chancre be situated under the frœnum, it could not be conveniently cut out; or on the frœnum, without dividing it; or on the glans, without the operation being severe; or behind the glans. But if the excision be put into practice, the virus might have previously escaped beyond that part; and as the object is too important for any doubt of certain security in the case, mercury would at the same time be given internally; and there would be a sore from the operation, which would require time for healing. Therefore, I look upon the method of excision to be something talked of, but which will never be in common practice.

**SECOND, OF THE APPLICATION OF CAUSTIC
TO ERADICATE THE CHANCRE.**

This is much more common in practice than the former, and has been adopted as long a time as almost the cure of the disease. The precipitate of

Vigo went to the same object. But Astruc speaks of the caustic itself; and Mr. Hunter also rather encourages it. I have applied it, but I now see so many objections to it, that I really think it ought to be condemned, and banished from practice. It must be remembered, that this application will only be safe, when it is applied to a recent Chancre, and a Chancre unaccompanied with an inflammatory disposition in the parts. But when there are many Chancres, and those on different parts, or when there is a disposition generally in the parts to inflame, the ulcer or ulcers produced by caustic may amount to a serious importance: and therefore my reasoning upon the question shall be confined to where it can be applied, and where it is most commonly applied.

If the question were entirely local, if the only object were that of an expeditious healing of the Chancre, it would not signify by what means that end was obtained, and that which did to a certainty dispose it to heal the soonest, would be the most preferable. But the whole of the importance of the object is still beyond that argument, for this is the moment when the constitution is to be guarded against the poison; and if the applied caustic does not ensure that beyond a doubt, which it most assuredly does not, then why should the patient be thus exposed to doubt and to danger? If the caustic be laid on and act, of course a larger sore is incurred, some time must be taken for it to heal, and when it is healed, there will, from the nature of the action

of caustic, be a hard cicatrix: this will perplex the understanding in determining whether the hardness be supported from the consequences of caustic, or the remaining virus which may be escaping into the constitution. But this is not all—both the stimulus from the caustic and the stimulus from the virus will irritate and enlarge the lymphatic glands of the groin, and then another perplexity and doubt will be raised, to determine from which of these two causes this symptom is incurred.

Who will take upon themselves to say, that the use of mercury internally may be spared, when caustic is thus applied, and that if it be spared, the patient will be as certainly safe?

From the knowledge that we have of the nature of venereal poison, it is impossible that the mere application of caustic alone can always ensure the patient from the future action of the virus. Then is not that method a wrong one, which does not procure a positive safety against future infection, and which does not set the mind at rest; and dissipate the dread of future danger? The idea, that from shortening the duration of the Chancre by destroying the virulent action which produces it, the duration of giving mercury internally will be, and may be also shortened, and the quantity also from that cause lessened, is founded not upon a sound knowledge of venereal action, and therefore it would be extremely dangerous to admit it. The Author who has asserted this idea has adopted a mode of

reasoning incompatible with the truth of venereal action. He asserts that the quantity of virus absorbed will be in proportion to the size of the Chancre, and to the time for absorption: that is to say, the action of venereal virus upon the constitution infected with it, will be more or less influenced, according to the quantity of that virus which is absorbed: as if an atom of foreign virus once imparted to the constitution, would not infect the constitution as effectually as an hundred or a thousand atoms: as if the most divisible drop of Small-Pox would not infect as effectually as the whole of the fluid extracted from a thousand pustules. Where would such a speculative idea as this lead us in the cure of a Chancre!

The question does not consist in the quantity of venereal fluid which escapes into the constitution, but whether any escape or not. There will not be less mercury required, because more or less fluid escapes into the constitution: that is the proportion of mercury required for the cure of a local infection, which produces such a change in the fluids of the constitution, as will necessarily oblige a cessation in the action of the foreign virus on the part to which it was imparted, and thereby also prevent a presumed constitutional infection.

The erroneous idea of this Author to whom I allude, and to whom I have so often alluded, arose from want of distinguishing the nature of action of foreign fluid on the subject infected by it. He rea-

soned upon a presumptive idea, that the constitution drew its infection from the secreted fluid discharged by a Chancre, and from that being absorbed: for he says, the quantity absorbed will be as the size of the Chancre, and the time of absorption, and the quantity of mercury must be correspondent to that. But surely, if this fluid will infect when it is absorbed, if this fluid be the fuel to bait the venereal flame in the constitution, a profusion of the same fluid would to a certainty excite other Chancres on the surrounding parts where it is lodged. This and the true nature of the action of it has been already laid down by me.—We are furnished with another proof, how necessary a true knowledge of the relative action of the poison is for the better ascertaining that which is necessary both for a cure of the local part infected, and for guarding the constitution against infection: from a false idea, that the constitution was infected more or less, according to the size and the proportionate discharge of a Chancre, the conclusion was—that there must be a local discharge existing prior to constitutional infection—that the mildness or virulence of the constitutional infection must depend upon the quantity of discharge—and that the mercury which may be necessary, must bear a proportion to the size of a Chancre, to the quantity which it discharges, and to the time for absorption whilst it continue to yield that discharge. Not one single iota of this speculative theory is founded in truth. I will repeat these

facts again, and conclude my argument: that the secreted fluid from a Chancre of any size, or of any duration, is the fluid of that subject by whom it was secreted, and is perfectly harmless to him—that a Chancre is the result of the action of venereal fluid of another subject—that this is the fluid which is not only acting locally on a part so as to produce Chancre, but is also the fluid which is absorbed into the habit, or which is presumed to be absorbing; and for the cure of which mercury is given, for the purpose of extinguishing its local action, and of course its constitutional action—that the quantity of mercury for a cure must not be regulated by the size or the time of the Chancre, but must be regulated by such change in the fluids of the constitution as will necessarily oblige a cessation in the action of the foreign virus on the part to which it has been imparted, and necessarily prevent its action on the constitution when any infection has been presumed to be absorbed.

THIRD, OF THE LOCAL APPLICATION OF MERCURIAL OINTMENT.

That has been recommended, both when the Chancre has not been previously extirpated, and after it has been extirpated or destroyed:—the mercurial ointment has been applied as a dressing to the part. By this it seems as if there were a con-

scientious mistrust, a mental misgiving, lurking behind the securities of excision and caustic, which have been thus topically taken. "However," says Mr. Hunter, "as our knowledge of the extent of the disease is not always certain, and as this uncertainty increases as the size of the Chancre, it becomes necessary, in some degree, to assist the cure by proper dressings; and therefore it may be prudent to dress the sore with mercurial ointment." These are Mr. Hunter's own words.

In order to be better understood in what I now mean to explain, I shall first impress upon the recollection, that it is not possible for me to object to the application of mercury, as soon as a Chancre is avowed: because I know, and my theory goes to prove it, that mercury ought to be immediately thrown into the constitution, for its preservation against the presumed progress of the foreign poison, as well as for the extinction of the local action of it; and that the mercurial effect must go on to a certain point, for obtaining both these ends. This I have said, for this is my doctrine: therefore I cannot object to mercury; on the contrary, I enforce its necessity. But I must confess, it strikes me that there is something very inauspicious or suspicious in the theory and practice of Mr. Hunter—and that neither of them are well founded, when we see such an application as mercurial ointment applied to the seat of a Chancre, or to the seat where the Chancre was, but from whence it has been extirpated, either

for a cure of the Chancre, or the consequences which it might have left behind it. There cannot be, in my opinion, but two reasons assigned for dressing that part with mercurial ointment, and these reasons argue an absolute deficiency in a true knowledge of the nature of the disease, and of the nature of mercurial action.

Mr. Hunter has assigned his reasons why he dressed the sore with mercurial ointment: it was so to act upon the part as specific mercury, positively to prevent that which may happen if it were not thus applied. This was one intention: and this must have arisen from a belief that a little mercury, applied on this part, would, both locally and preferably, operate against the virus, by being approximated to it, if any virus remained: that it would preferably operate upon the presumed progress of the virus into the constitution, by being also introduced into the constitution through the same channel as the virus was. I could be as soon induced to believe any other impossibility in Nature, as be brought to believe that mercury, merely as specific mercury, has any influence upon venereal virus, in any form or denomination whatever. If the one and the other were mixed together, they would, so mixed, retain in full effect their original properties. But the local application of the one for the local extirpation of the other, or for the local security of a part that was infected by the other, argued at least, that he was not wholly disposed to surrender up

such an idea. And the consequent idea of supposing, that by sending mercury after the presumed absorption of virus, and by the same channel, has as narrow a foundation in reason (if it be any thing like reason) as the former idea : for it looks at least suspicious, that the former idea was pursued, and that it was the same theory which led him to promise something particularly specific, in dressing a part locally with mercury, for the object of extirpating the remaining virus, if any did remain. From want of a true knowledge of the nature of the action of mercury, the practice of dressing a Chancre with mercurial ointment has been long in use ; but it is so dissonant to reason, that at any rate, if any other dressing be at hand, it should not be applied, lest by such a practice it may falsely be presumed that it was done with a specific motive.

THE FOURTH METHOD OF CURE.

The most important points by which a Chancre is judged to be a venereal Chancre or not, are from an enquiry into the nature of the connection by which it was supposed to have been contracted ; into the date of the Chancre being contracted, and since its first appearance ; and into the condition of the state of the PENIS, on which the supposed Chancre or Chancres are discovered. In a very recent Chancre without such enquiry there is nothing

by which, *prima facie*, it can be pronounced to be positively venereal. RECENT ULCERS, NOT VENEREAL, ARE EXACTLY SIMILAR TO RECENT VENEREAL CHANCRES, IN EVERY STATE AND CONSTITUTIONAL CONDITION OF THE PENIS.

Prima facie, from all that I have ever read or seen, or from all that I have ever been taught from tale or history, I have not by any sign been able to distinguish an incipient Chancre venereal, from an incipient ulcer not venereal. The incipient Chancre and incipient ulcer will be ever the more or less virulent, according with the idiosyncrasy of the subject thus conditioned, and it is very possible that both may be healed without the smallest application of mercury, or that both may assume a malignant disposition.

Not being able, as some modern Writers are, *par excellence*, to decide upon the true nature of a case but from previous inquiry; and even then, not being able, as some more enlightened modern Writers are, to decide positively, but from previous enquiry, and not even then, not being able always to decide, I proceed to consider that sore as venereal Chancre, or as ulcer not venereal, according to the evidence the history of the case offers, which I may have before me. One consolation however, amidst the varieties of opinions, and particularly so lately disseminated, is, that in a recent sore, wheresoever situated on the Penis, mercury, if given in the very first instance in small quantity, can do no harm to the constitu-

tion, and if it could, it can be withdrawn ; whereas, if the sore should prove to be venereal by assuming callous edges, and a thickened base, more mercury will be afterwards exacted for the cure, come when it may. Another consolation to the patient is, that if the cure of a sore be commenced by the administration of mercury, as soon as the sore is discovered, there need not be any stimulating dressings applied to the sore, by which an opinion of the true nature of it might be made more obscure and difficult. For when once the doubtful sore is dressed with any thing stimulating, it is then no longer a sore as it was, but a sore as it is by being so dressed. It is better to dress the sore with lint, and let the doubtful sore disclose itself.

Besides the doubt which always ought to be thought on, in a recent sore, and where nothing decisive can be established, there is an objection to the laying mercurial ointment on a sore of such an organ as the Penis is composed of, which I judge to be self-evident to any man's comprehension, that mercurial ointment will very often inflame the part, and especially in an inflammatory case, and if it should, there would be, instead of a cure, an erysipelatous inflammation to contend with into the bargain.

As in all probability such a Chancre (if it be one) might heal under the mildest treatment most readily, I should recommend no more than five grains of *Pilulæ Hydrargyri P. L.* to be taken every night, till the sore disappear; or if that quantity of mercury

should not be effectual, it must be gradually increased.

There will not, nor there cannot be by this method a doubt entertained but the constitution will be also safe from future infection, and there will be no future inducement for suspecting that the disease is left behind, and that it may constitutionally appear hereafter. Whereas the case will ever be doubtful and protracted, by its being made more complex — by any hardness remaining on the original seat of the Chancre — by hardness from the action of caustic, or by applying mercurial ointment—or by enlargement of the inguinal glands, which the inflammation from caustic particularly will excite. The process by caustic, or caustic washes, is neither safe, decisive, nor final :• more mercury may be given than is necessary to be given where the Chancre is left undisguised; and the difficulty of healing the part destroyed by caustic will be fully as great, the time as long, as that of healing the original Chancre. When a Chancre heals by the action of mercury on the constitution, that method is sure to be final, and the constitution is sure to be safe : so much cannot be said when a cure is conducted by any other process.

ANOTHER DEGREE OF SUPPOSED CHANCRE
FROM VENEREAL INFECTION.

The generality of patients do not apply to Surgeons until the Chancre appears so self-evident as to leave in their opinions no doubt of its being venereal — for they connect that opinion from former connection. The time most usual is about a fortnight after the sore first appears, and about twenty days after a doubtful connection, when the Chancre is spreading, the base of it thickening, and the edges of it becoming callous. But if there be no appearance of inflammation more than belongs to the nature of the case, a similar treatment to that I have already offered will be the system eventually for the most permanent cure. To proceed with the pilul. hydrargyri in small doses, for at least a fortnight before any dressing be applied to the Chancre, excepting dry lint, after washing the Chancre with milk and water, or something else as mild. At the expiration of a fortnight, in a simple case, the Chancre under mercury, and where the patient tastes the influence of it upon the constitution, will begin to discover a change; to cease to spread, and to commence to heal; in that case, to assist in cleansing its aphthous base, such a topical application as a solution of vitriol, or calomel with lime water, will accelerate its healing. In such a simple case as this

it will be prudent to continue on the mercury at least a few days after the healing of the Chancre, and more mercury should be given to promote the cure if necessity indicate it.

ANOTHER DEGREE OF SUPPOSED CHANCER FROM VENEREAL INFECTION.

When virus is imparted to a constitution already inflamed, there will be a necessity for attending to the previous inflammation, as well as to the specific inflammation excited by the action of the foreign poison. In that case, not only the constitution but the diseased part will exact assistance—will exact that which is best to be done for the vitiated state of the constitution and the Chancre before it can be brought to granulate and heal. The vitiation of the fluids from intemperance and continued irregularities, together with omissions after the Chancre is contracted, is the cause of all the untoward symptoms which arise in Chancre, and which obliges the patient, in the end, to retire to his chamber, and to adopt a stricter discipline than would have been necessary without such provocations.

Whenever the parts betray this vitiation, it must not be imagined that a change for the better will be sudden; it must not be concluded, that the most prudent correction of the constitution which can be adopted will effect a change of such vitiation, with-

out time being allowed: so that this condition of the constitution, and the aggravated condition of the Chancre also, will be objects throughout the cure. Moreover than this, there might have been a prior taint in the habit, and a Chancre from this might be the more obstinate, knowing that a subject constitutionally infected can contract a local infection.

When a simple Chancre or Chancres, independent of any other inflammation but the specific inflammation, have not been attended to, and when mercury has not in the first instance been given, such Chancres will proceed slowly to eat deeper and deeper, and their circumference will be as slowly enlarged. To the shining inflammatory red appearance, which was the incipient appearance, will succeed an apthous appearance: the treatment which I have laid down already, is always that treatment which will be necessary and proper, only with this regard, that mercury must be gone on with until the Chancres heal, and for some days after; and by its effects the requisite quantity of it will be ascertained better than I can, a priori, prescribe it.

The usual appearances which indicate a compound case are, Chancres acting very rapidly, spreading unusually, of a livid sloughing aspect, of an irregular form, eating very deep, and proceeding to destroy the parts with uncommon rapidity. The appearance of the Penis will be tumid, the prepuce thickened, the glans swollen and hard, the discharge from the sebaceous glands profuse, and from the

Chancres will flow a bloody sanies. There will also be generally fever attendant upon those symptoms. The two first points indicated are, resisting the natural vitiation, and resisting the venereal progress. As neither of these means are to be immediately accomplished, an aggravation of all the symptoms will naturally go on, until a correction of both be carried into effect.

The patient must therefore submit to stay at home. If his pulse indicates it, he should be bled in the arm, for I do not trust leeches to an inflamed Penis. He should take an opening draught, and observe the strictest regimen. His pores should be opened by James's Powder, and mercury should be begun with as soon as possible, especially if hitherto none has been given. A drachm and a half, or two drachms, if the febrile symptoms will permit it, should be rubbed in every night for ten nights: and then the result should be sought for in the appearance of the Chancre. The propensity to sphacelation should be resisted by every means, and the local treatment should be all to this regard; if the inflammation should not abate, the patient should be bled again and again. The Penis should be fomented with a strong decoction of poppies, to two thirds of which a third of spirit of wine should be added. Linen dipped in liquor ammoniæ acetatæ should be wrapped round the Penis. The Chancre, if the prepuce permit it, should be washed with warm water, and the Penis should be immersed in warm

water, during the act of urining. These means generally will not, with a submissive patient, fail of success; but, if the swelling of the prepuce forbid it, an injection of warm water should be thrown up. The Penis should not hang down.

If the seat of the Chancre be on the prepuce only, it is not so material to divide the prepuce; but if the seat of the Chancre be on the glans, and there are apprehensions that the ulceration is still proceeding, and that an hæmorrhage will ensue, the prepuce should be divided, and the Chancre exposed. Mr. Hunter has recommended an application of turpentine for an hæmorrhage from a Chancre, for it must be observed, that sometimes an hæmorrhage there is serious.

But if the treatment be later adopted, and if mercury be not given at all, or so given as not to produce its necessary action, if the constitution has not undergone the preparatory means which I have exacted, and if, in consequence, such unfavourable symptoms are advancing more forward, still the same means are to be pursued, only with additional diligence. But if, under all these adopted means, after the expiration of a fortnight or three weeks, evident symptoms of convalescence do not appear, but a contrary effect is observed from the action of mercury on a constitution thus conditioned, time must be given for restoring the constitution, for assisting the state of it, by pure air, nitric acid, bark,

milk diet, and by every means which tend to take off the irritability arising from a habit thus abused.

The symptoms, although they have not been observed to give way, will not be so rapid and alarming in this stage of the cure as they were at first, and after that which I have laid down has been adopted. The condition of the constitution being so restored as to receive that full impression from mercury which is necessary for the cure of the venereal part of the case, and the time having been allowed for bringing it into that condition, through pure air, wholesome food, observance of temperance, and the assistance of restorative medicines, nitric acid, bark, &c. then by returning to the mercurial course it will be found to be effectual: the Chancre will heal, and every venereal constitutional symptom will be hereafter out of the question, provided that mercury, when it be found capable of acting, be not negligently and slovenly applied, or that it be long enough persevered in for obtaining that necessary end.

ANOTHER DEGREE OF SUPPOSED CHANCRE FROM VENEREAL INFECTION, INCLUDING PHYMOSIS.

In all cases it is absolutely necessary to ascertain as well as possible the cause of an ulcer on the Penis, without which it would be impossible to know how to treat it. When I have not been able

to ascertain this, and when the case, notwithstanding, has been ultimately under my observation, I am naturally more at a loss how to decide, for if I see a case of Phymosis, without having seen the case before it was a case of Phymosis, I have nothing to trust to but the relation of others. I think Phymosis is as serious an attack, whether it arises from a venereal cause, or from any other, as any in the catalogue of venereal symptoms and other causes. It might destroy glans, it might ulcerate urethra, it might be the cause of the sloughing of scrotum and prepuce. It might oblige the urine to pass through a diseased opening, which no Surgeon can afterwards heal, and it might be the cause of the Penis sloughing altogether.

These observations induce me to remark, that to define the action of a Chancre which brings on Phymosis, it is requisite that the Chancre should have been previously seen, before it be determined that the Chancre is the cause of Phymosis.

A CASE.

A Gentleman, aged 27, showed me a Chancre contracted a fortnight before. He was, though a married man, always out. I gave him pills, five grains in each of *pilulæ hydrargyri*, and I desired him to wash the Chancre with warm water. The seat of the Chancre was close to the *frœnum*. It

was not before a fortnight that I saw him again, when I found the Penis swollen, the skin of it inflamed, the Prepuce so screwed that one could hardly tell the way to the glans. He had a full pulse, and the point where the latent Chancre was concealed, was extremely tender to the touch. This was the state of the patient, only after three weeks or little more of the contraction of the disease. I bled him in the arm, gave him James's powder, applied cloths dipped in liquor ammoniacetatae constantly fresh; and I injected warm water between the Glans and Prepuce; I also enjoined him to immerse the Penis in warm water whenever he solicited to urinate, and I would have drawn the urine off by catheter, if I could have done it, being convinced from what I have seen in practice, that nothing that can happen to make a sore become a bad ulcer, more than the lodgment of urine, and that whoever thinks otherwise must have had no experience in practice. I repeated five grains of James's powder every night; I gave him no opiate, for I do not apply to an opiate in a case of true inflammation.

I kept him almost literally upon bread and water, and I bled him twice more at the intervals of forty-eight hours. I acted so, knowing the constitution of the patient, and the TRUE nature of the case, as to have made it more surprising, if it did not get well, than it would be, if it did get well. And this is the best mode of treating dangerous inflam-

mation. In the course of three days after the patient was confined to his chamber, I began to give him the *pilulæ hydrargyri*, five grains morning and night; I did not do this from choice, but convenience. I should have preferred rubbing in the *unguentum hydrargyri fortius*. In the process of a week, by all these means, the violent symptoms began to give way; a plenteous discharge passed from the diseased glans. More access could be had through that distorted aperture of the Prepuce; I could swell the space between it and the glans with an injection of warm water. I could not prevail upon the patient to take more than five grains of the *pilulæ hydrargyri* twice a day, and therefore, without his knowing that he took more mercury, I joined two grains of calomel to his five grains of James's powder, which he took every night. Every day convinced me that the case was gradually mending. The discharge thickened, the progress of the disease was stopped, the Penis was paler and lighter, the painful sensations were abated, the ulceration of Prepuce changed to a more favorable aspect, the seat of the Chancre could bear to be handled; and without any thing more than a repetition of the means already described, injecting lime water with calomel, instead of warm water, continuing on the *pilulæ hydrargyri* twice a day, the patient was discharged in the course of six weeks.

I have only to add, that if the means I have stated had not been successful, I should have given

a grain of emetic tartar with the James's powder, as nothing tends to abate inflammation excepting bleeding and to put an end to its mischievous progress more than vomiting.

If ever there was a case where time is more precious to prevent the ravages of inflammation, I know not where to find it. The stake contended for being, at least, the preservation or loss of the Penis.

CASE II.

A Gentleman, aged 25, showed me a Chancre situated under the frænum, which, upon inquiry, was established to have originated from venereal virus; I supplied him with pilulæ hydrargyri, and desired him to dress the Chancre with dry lint. In the course of a week he went to Newmarket, where he remained a week longer. On his return, I found him in a most pitiable state. Phymosis, accompanied with high inflammation, and, notwithstanding I pursued the same course I did in the former case, to put a stop to the rapidity of the disease, I did not succeed before the ulcer had penetrated the urethra. I attribute this to the urine having been extravasated and confined for want of a passage. Very fortunately the prepuce sloughed before the glans was entirely destroyed. I drew off his urine by the gum catheter as soon as I could, and this put an end to the progress of sphacelation:

and from that time the case began to assume a favourable complexion, and without any further progress of destruction; the parts healed under a very moderate proportion of unguentum hydrargyri. But there had been such a loss of the substance of the urethra, that it could never after be brought to unite. Indeed, what was attempted by myself and others, made the chance of it more remote. He passes a silver instrument, formed like a bougie, two inches in length, flattened at the end, and fitted to the aperture. This secures the opening for the passage of urine. The remaining part of the glans swells in erection, but neither semen nor urine passes through it.

CASE III.

A Letter-carrier, in a walk of St. James's, came to me with Phymosis from Gonorrhœa. Circumstances put it out of his power of remaining all day at home. He was bled freely, and cloths wet with liquor ammoniæ acetatæ were wrapped round the Penis. With a great deal of difficulty a very small catheter was passed into the bladder, and I made him practice the introduction of it himself, and not suffer, if he could help it, the urine to lodge between the prepuce and glans. He was very successful, and without any further obstacles, the Phymosis abated. He took five grains of pilulæ hydrargyri every night for

a month. Half a year afterwards he called to complain that although the tumour had completely subsided, yet he could not denudate the glans. By passing a probe round the glans, I found that it had formed an imperfect junction with the internal surface of prepuce. We both agreed to leave it as it was.

ULCERS ON THE GLANS FROM CAUSES NOT
VENEREAL, WITH WARTS.

There are so many causes for ulcers on the glans, that discrimination cannot be too minute upon this serious and important question. Ulcerations on the glans, without a venereal cause, are much more dangerous than a venereal Chancre. They are generally connected with the state of the urethra*, and always owe their origin to the urine acting upon the Penis. It is generally from this cause that warts are found to infect the glans, and that there is such a difficulty about them; for it is not difficult to get rid of warts, if at the same time that which was the pabulum of them was gotten rid of also. So long as the Penis is diseased, the warts, after they have been removed, will be succeeded by others. Scattered urine, and redundant secretions will generate warts. Therefore, upon all occasions, the cause of them should be detected; and perhaps, in that case,

* In Part II. of *Vesicæ Lotura*, by JESSE FOOT, there are many cases of Phymosis, from page 59, and onwards.

one might be led to the detection of that which otherwise might have escaped observation. Under the long continuance of irritation from stricture, where the urine dribbles away, that will encourage the growth of warts; also the redundant discharge from the sebaceous glands; also, where the smallness of the aperture of the urethra hinders the urine from passing out of the urethra as fast as it is supplied by the bladder; these causes will keep the Penis in an habitual state of disease, and furnish ample evidence of the mischievous effects of urine thus dribbling between prepuce and glans. These causes will not only produce ulcers which, unless they are detected, the ulcers will be incurable and the warts also. To get rid of warts, if they be very redundant, it is best to shave away as much of them as one can, and by that be able the better to root them up altogether. Sabin powder will succeed, but not without its being applied more frequent than it usually is, for as soon as it becomes moist it loses its effect. A solution of arsenic does the business better.

Having referred to cases on Phymosis already published by me, leaves me to say the less upon causes and consequences now.

CASE I.

A Gentleman, aged 25, very lately applied to me, and I found him in the following situation: he had

a very inflamed and tumid Penis, a profuse discharge from the urethra, ulcerations on three parts of the glans, the prepuce thickened, the aperture of the urethra so small as not to admit of a bougie more than a third of the common size of the larger ones. He urined before me, and he scarcely made a stream, and what passed was in a scattered manner. He had eruptions on his forehead, and appeared as if his constitution was very much distressed. He had on the ulcers some red precipitate ointment, spread on lint. He told me, that these eruptions on the glans had been there for some time, and that every effort had been made to remove them to no effect. He told me also, that being in the way of consulting many of the faculty, he had consulted them not from day to day, but from year to year. I asked him if before he had any cause assigned for these sores on the glans, or for the inflamed state of the Penis, or for the small and scattered stream of urine, or for the eruptions on his forehead. To all these questions he answered no. I passed a small bougie home to the bladder, and found the urethra particularly contracted from its aperture through the glans. I removed the precipitate dressings, and substituted a wash, consisting of five drops of the vitriolic fluid to an ounce of water. I enjoined him always to urinate with the Penis immersed in warm water, and to call again in a week. The ulcers were then in a healing state, and when he called the week after they were healed. The

patient being by this time assured of the probability of success, listened to my proposal of dilating the aperture of the urethra, and it was done after the following manner.

Take a piece of small common bougie, about four inches in length ; split two inches of the four inches at the larger end of the bougie, and between the split parts fasten the blade of a lancet. The shoulders of the lancet will be broader than the bougie, and will be exposed only for use, which the point of the lancet will not. The bougie unsplit will lead the shoulders of the lancet in the right direction, and the aperture of the Penis and throughout the glans, the blades of the lancet by being pushed forward, will thus effectually enlarge all the urethra annexed to the glans, and restore to it all the advantages of a sound state.

By dipping a bougie in oil, and passing it about two inches up and down, so as to keep the divided lips apart till they are brought to heal, this part of the case was perfected. This being done, the state of the urethra was examined, and a stricture of about seven inches up was detected, and which in process of due time gave way to as large bougies as are commonly passed, for henceforth there was no obstacle to the passage of the largest ones. This case being thus put into train, and the whole as far as it appeared promising success, the bougies were passed in increased degrees, the discharge ceased by the use of a vitriolic injection, and no trace remained in

the course of three months of a complaint. If the cause of it had not been detected, it must have terminated with the destruction of the Penis at least. I now come to treat on the eruptions on the face, these, by continuing nitric acid internally, and applying to the eruptions unguentum hydrargyri nitrati, mixed with spermaceti ointment, disappeared also. There had been a consonancy betwixt the eruptions on the face, and the state of the urethra. The eruptions on the face, and also on other parts of the skin, had appeared many years before, and as long as the course of the urine suffered a check from the stricture in the urethra, and from the smallness of the aperture of the urethra, so long I conceive the eruptions which originated from that cause (in my opinion) would have remained, in spite of any thing proposed to remove them. These all disappeared. And now it only remains for me to give my reason why these eruptions should have a consonancy with this case. Whenever the urine is haulted, by its not being able to be discharged after it has been excreted out of the habit, and lodged in the bladder, if the urine should not from any other cause besides this now treated on, be able to obtain an easy passage, the constitution will suffer from it. More urine will necessarily float in the habit, and thus the habit will become vitiated, and what cannot be excreted must continue vitiating the habit. The passage of urine having been obstructed, the urine had no way of being regularly excreted, and from that

cause recoiled upon the constitution ; the passage of urine having been let free, the constitution was relieved, and the eruptions disappeared. I am disposed to think, that most of the destructive cases which occur, where the glans participate in them, and where the progress of sphacelation cannot be stopped, originate from some such cause as the case here described. In many instances Phymosis shuts out the cause, and therefore nothing else can be thought on but Phymosis, as nothing else can be seen to direct the judgment. And even in those cases where the supposed Chancres are seen, and where mercury has been given for them, and where the sphacelation of Glans, Prepuce, and Penis, has been presumed to have originated from mercurial action, I am disposed to think that, although the mercurial action might have accelerated the destruction of the parts by being applied to a case it could not cure, that the case would have terminated in the same manner, although perhaps not with the same rapidity.

CASE.

A young tradesman applied to me with Phymosis, and extravasated urine in the scrotum. It was in this state I found him. He had been attended by another surgeon up to this time. He appeared in very great distress, and had all the symptoms attendant on high inflammation. The Phymosis, from

what I could collect, had preceded the suffusion of his urine more than ten days; and every day the Penis, from the beginning of the attack, had assumed increased degrees of inflammation.

The swollen state of the Penis, from high and constant inflammation, had contracted the circumference of the urethra*, so that the urine could not

* According to Mr. Travers, such an explanation as mine must be exploded; for MONSIEUR BISHAT has explained the same thing, which he thinks is done after a better manner, and therefore brought it from France (mark it)—“and that swelling of the Parietes necessarily diminishes the calibre of a canal.” Walls and Calibre, in anatomy, sound strongly ungrateful to the English ear. Round the city of Babylon, or in Woolwich-park, they would be much more a-kin. But in different and distinct ages, this corruption of words has not passed unnoticed. My objection to this Babylonish jargon is the humblest of the present and of the precedents of former ages.

In the 4th scene of the fifth act of Shakspeare's Hamlet, OSTRIC, the Dandy of his day, bringing a challenge from LAERTES to HAMLET, calls “HANGERS” (which were only side arms) “CARRIAGES.” HAMLET tells him, that the phrase would be more german to the matter, “IF WE WOULD CARRY CANNON BY OUR SIDES.”

Sallust also has recorded from the speech of CATO in CATALINE'S CONSPIRACY, that the quantities of Dandies of that day had perverted and even lost the true use of words.

“NOS VERA RERUM VOCABULA AMISIMUS.

Cato apud Sallust.

“CALEBYRE AND WALLS!” “WALLS ROUND ULCERS, AND WALLS ROUND ANEURISMS.”—Such are the ebullitions of *Genius*, the boilings over from the furnace of Nature's work-shop, the PINEAL GLAND, or the waste of the dust of a diamond whilst it is polishing by an artist!

pass but in a diminished and dribbling stream. The urethra at length ulcerated about three inches from the glans, just where it passes over the scrotum, and that part of the Penis distinguished the seat of the opening by its being hard as cartilage. He was bled profusely, purged, and large doses of James's powder were administered, which both sweated and vomited him. Cloths wet with liquor ammoniæ acetatæ were kept constantly round the Penis. I did this preparatory to the effort intended to pass catheter into the bladder, knowing if successful, the Penis was safe. Twelve hours from the time he was first seen, the trial was made with the smallest gum catheter in which there was a stillet. To avoid that tortuous twist of the prepuce, which is so commonly found in Phymosis, it was stretched into as strait a line as it could be, and the catheter, after a few attempts, was passed into the bladder. The catheter was left till the next morning; and the patient, who was very handy, took care that it should not escape. The progress of more mischief being thus stopped, the suffused urine had already inflamed the scrotum, and in the course of a week there was fortunately a sloughing of scrotum in a right direction to the ulceration of the urethra. The catheter had been ventured to be withdrawn on the second day after it was first passed. And the young man could pass it himself, and continued to do so, for three months, during the cure. The dressing applied was sometimes lint only, and some-

times tincture of myrrh, which agreed with the case. The Phymosis soon vanished, when it was no longer provoked by urine. The worst symptom to contend with was the cartilaginous feel round the Penis, where the urethra had burst. In order to dissolve it (for without dissolving it the division in the urethra would not close) ten grains of unguentum hydrargyri were every night spread on lint, and laid on that part of the Penis. This had a good effect, it gradually melted down the induration, a discharge from the scrotum of matter well conditioned, and in encreased quantity, followed. The discharge gradually lessened, till the whole healed.

CHAPTER XIV.

ON THE CURE OF BUBO.

I HAVE already premised—and the experience of every day's practice confirms the fact—that an enlargement of the inguinal gland or glands will be often the first indicative symptom of venereal infection: and I have already premised—that it is the foreign fluid only, imparted by another subject, which, by being absorbed, acts and contaminates the constitution—that this poisonous foreign fluid sometimes leaves local impressions, at the commencement of its progress into the constitution—

that these local impressions are not a *sine qua non* of venereal infection—that the constitution may be infected, without a single instance of such local impression—that the discharge from these local impressions is merely a result of the action of the foreign fluid—that that discharge is a secretion proper to the subject who secretes it—and that any absorption of it is of no importance, nor has any reference to that poisonous infection of another subject, which does proceed in contaminating a constitution. Nothing can confirm these truths more strongly, than that this local impression is arbitrary—than that the disease may yet go on—than that the constitution may be as effectually infected without there being any local discharge from any local action of the foreign virus to be absorbed, as when there is discharge in consequence of this local action of the foreign virus, and when it might be absorbed. For if absorption of our own fluid supported the disease, and if the urethral membrane had not an absorbent power, there would be always enough of it lodged upon every part out of the Urethra, which we know does absorb foreign virus, and which we know does thereby contaminate the constitution. The same may be said of the fluid of a venereal Bubo, and the same of the fluid of a Chancre.

When a Bubo arises in consequence of a previous local infection, which will be either Gonorrhœa or Chancre—that Bubo may be known to be venereal: but when it arises without such primary impression,

and without any other apparent cause but a suspicious connection—that will be a sufficient inducement for determining it to be from a venereal cause: for I believe inguinal glands seldom come to abscess but from a venereal inducement. And when there has been a suspicious connection, reason would not lead one in search of another cause.

When Lues Venerea has completely tainted the habit, when the constitution is in consequence labouring under the disease throughout, when it is in an hectic state, and when the habit is wasting fast, this is not that part of venereal effect which I am proceeding to treat, but it is purely the local impression of the foreign virus upon inguinal glands from recent connection.

There are two points which are most materially to be discriminated—the anatomical nature of the part, and the nature of the stimulus:—that it is venereal virus acting upon and disturbing a glandular body, and aiming through it to obtain a passport into the constitution;—and that the knowledge of treating glandular inflammations and glandular indurations, be the cause what it may, is not so certain, and is not so well defined, as the same knowledge of treating other anatomical parts badly conditioned. A gland, once disturbed, does not possess that direct and ready disposition for recovery, which other parts possess, after the cause which disturbed it has been completely removed. The management therefore of a Bubo must be regulated

with a continual reference to the nature of the part, and to the possible power which the foreign virus has by it of infecting the constitution.

It must be observed, that a gland may, when once diseased by venereal virus, afterwards be continued in disease, from too long a continuance in mercury ; and, that because a gland does not yield to the mercurial action which is calculated in general to be sufficient for removing venereal actions on other parts locally infected, that from hence more mercury will be often wrongly employed in the cure of a Bubo, than will be necessary for curing the most confirmed venereal symptoms. And it must be observed, that from an increasing sedulity, arising out of the obstacles which offer in consequence of venereal Bubo, more is often done than is necessary to be done ; and often that which is done for promoting a cure, instead of answering that end, continues the disease, and produces every contrary effect to that which was solicited. Every days observation convinces me of this truth : and the same observation shews to me, that the common people never experience so many distressing situations from the venereal Bubo, as those do to whom mercury is more profusely given for dispersing it, and to whom, when it be not dispersed, mercury is still given to the end of the case, or as long as the patient will permit it, seeing that his case is growing worse and worse by it.

The application of mercury, previous to a Bubo

coming to abscess, for an intention of dissolving it, demands a particular consideration from each particularity in the symptoms attendant upon the Bubo. For if, from a certain idiosyncrasy the Bubo inflame very rapidly, if the fever run high, if the pulse be extremely quick, and if the stimulus excited produce at night profuse perspiration, I do not think that is a favourable state for any fair promise of success in the action of mercury for dissolving such a Bubo. The additional irritation which the mercurial stimulus will excite, the time which this stimulus will require for effectual action, the obstacles which it will meet in that action, from the venereal stimulus already in action, must terminate to the prejudice of the constitution of the patient, and of a cure of his Bubo. For it must not be forgotten, that a regard should all along be paid to the condition of the patient. If a profuse application of mercury should, in this stage of the case, fail of the intention, and if the Bubo (which is most probable that it will notwithstanding) should come to abscess, a foundation then is laid for future malignant symptoms, and for future doubts whether mercury will hereafter be necessary or not. The case will be protracted, and be made more dangerous and complex, and the patient will be reduced, not benefited.

When a Bubo betrays signs of coming rapidly to abscess, and sooner than the action of mercury upon the constitution could take effect, we are assured that the fever will subside with the formation of

matter—and when that is discharged, the mercurial process may fairly be began upon, the action of it can fairly be ascertained, and the effect in consequence will be fairly defined ; less time will be lost to the patient in the cure, and no dread or doubt of necessary or unnecessary perseverance in mercury will be confounded in the question.

If there were no such distinctions as these, the same means that would dissolve one Bubo would dissolve another ; the same fever would be attending upon each, and the same result would be conclusive for all. But, independent of these reasons which I have produced against the probability of mercury dissolving a Bubo, when it is thus acute in its consequences, and which are the result of practice, I have every reason to urge the same arguments from the result of true theory ; not only from my general opinion of the action of mercury as a powerful stimulant, but from that particular distinction which I have made between the pure local action of mercury upon the local action of virus, independent of constitutional action, and which I deny to be any action in any way relative to venereal virus ; for that action must consist in its constitutional affection. Therefore, if mercury cannot be thrown into the habit before the Bubo has already come to abscess, *cui bono* should mercury then be administered ?

Having stated, that when the symptoms of Bubo are truly inflammatory, and in consequence of which it will necessarily come to abscess, and having urged

from that cause not only the improbability of preventing it through the action of mercury, but also the pernicious effects of an ineffectual stimulus which would arise from mercury thus applied, so far as it regards the promotion of future embarrassments in the case, I shall proceed to the treatment of a simple Bubo, and which is not attended with such inflammatory symptoms. Mr. Hunter, in the same page where he has avowed a positive certainty of success by the treatment which he had adopted, also avows, that after every known method has been used, Buboes cannot, in all cases, be dissolved, but come to suppuration. And as we all know that it is not in human skill to dissolve every Bubo before it comes to abscess, so we are all convinced that there is not a method yet known which will or can to a certainty effect it. The rapid progress of a Bubo, and the fever which accompanies it, appear to me to be the only obstacles against attempting the cure of a Bubo before it comes to abscess. And therefore, I see no other reason why it should not be attempted, for if the attempt succeed, time will be saved by it; as it will also when the Bubo is sluggish and hard, and when, without the virus being destroyed, it will be likely to remain so, until other venereal symptoms may follow from it. But the method for effecting this should be attended to, and in my opinion it is of no little importance.

Whatever virus be lodged on the gland which is the seat of the Bubo, cannot be influenced, as a local

poison, by mercury, unless it be influenced upon the same principles, the same regulations, and the same true laws of mercurial action, by which other local parts, venereally affected, are influenced, namely, by the action of mercury upon the constitution—by the change alone which it produces upon it: and as the gland which is the seat of the Bubo will only be additionally stimulated by a passage of mercury through it, without locally destroying the virus, it therefore is not only preferable but just in true theory to introduce the ointment into the habit by the other thigh, that the gland may not be vexatiously and vainly irritated, when the object is that it should not: for if the gland be not provoked by any other cause than venereal stimulus, so when the part is freed from that, the tumour may and will the more readily subside. In the cure of a Chancre, no one presumes that a local application of mercury will produce any friendly effect; on the contrary, it will only aggravate. It has been tried time out of mind, and it has been abandoned from experience. Then why should it be presumed that mercury will effect more on one local part, towards the destruction of virus, by chymically neutralizing the virus, than it will on another? The reason is still the same, and as strong against the possibility in one instance as in another.

But so far from the action of mercury being certain in reducing a diseased venereal gland, under the most favourable mode of applying it, the event

will always be uncertain. And this uncertainty in the event is always dependant upon the idiosyncrasy of a patient, and the state of his health at the time of the application of mercury. For if the stimulus of mercury inflame the Bubo, or if an increase in its quantity for bringing on the necessary mercurial change should prove to be requisite, then by the former a sluggish indurated Bubo will be brought to abscess, and by the latter it will remain obstinate in its sluggish state, for a longer time; the constitution of the patient temporarily suffering an ineffectual stimulus from the mercurial action.

To ascertain how far it be safe, prudent, and justifiable to urge the action of mercury for dissolving a Bubo, and curing the disease, without the Bubo coming to abscess, the mercury should be administered with the same expectations as in the case of a Chancre, and with always a reference to constitutional conditions. Leeches applied on the swollen gland. Six grains of James's powder to be taken every night, and if a vomiting be not excited, half a grain of emetic tartar should be joined to the powder. The bowels should be cleansed by mild purgative salts, and cloths wet with liquor ammoniæ acetatæ should be constantly applied. The similarity of the state of the patient will exact similar effects from mercury in each complaint: when the case is simply venereal stimulus upon a pure constitution, and that in its coolest state, as much salivary effects from mercury as will cure a Chancre will de-

stroy the venereal stimulus on a gland : and at any rate, if such effect does not succeed, mercury should not be gone on with ; but rather the patient should do nothing more than endeavour to amend the habit, and leave the future consideration of his case to future eventual circumstances. But it cannot be conceived how many Buboes will subside, when the mercury is withdrawn, after the inflammatory stimulus by it has been perhaps supported from a contrary expectation too long a time.

My intention is to impress these truths—that the venereal virus will in this instance exact no greater effect from mercury, than for the cure of any other local complaint — that the case is a glandular affection — that the stimulus from mercury will inflame glands — that when the stimulating cause is withdrawn, glands will subside with more facility by the patient being restored to a state of health, and by his living in such manner as to promote and support that state of health — that there is not essentially so much to be dreaded from the original cause, which is the venereal attack on the part, as there is from a continuance in mercury after a cessation of its requisite good effects—and that too much sedulity in reducing the gland, through mercury, has brought on all the bad conditioned Buboes, which are found in practice, or marked in the history of cases.

A prevention of all these bad conditioned cases is more important by far than a cure of them. Precautions must be taken, in the first instance, for

preserving the constitution from being exposed to malignant consequences. A right beginning will preclude a wrong event; so will the neglect of it oblige us hereafter, when these bad conditions do obtain, to desert the wrong and begin with the right. If a patient with a Bubo has lived freely, if his habit be heated and inflamed by drinking and other irregularities, these should be corrected, and the effects of them repaired. It is always an object, that mercury should have no other stimulus to contend with but the venereal. A system in practice—a true observance of that system—a strict regard to the patient's health—and patience in observance of the indicating intentions in the constitution, will do much for the cure of a Bubo. As little mercury as can be only necessary, will only be justifiable—to discriminate when its good effects cease, and its bad commence, is a line not difficult to be ascertained, when the truth of such a natural event is known and attended to. In short, it will be much safer to give too little than too much mercury, in the diseased state of a Bubo: the latter cause will be more tedious to be removed, and more dangerous than any venereal symptom, purely as venereal, which may hereafter occur. The constitution for the cure of a Bubo will exact particular attention—for it must be remembered that the symptomatic fever is often of serious importance, and that time and proper treatment should be exacted from that cause. I have seen a patient as much reduced by an inflammatory

Bubo, as he could have been in the same given time by a consumption. Then, without amending such a state, is there any good to be hoped for from mercury? Certainly not*.

I shall now proceed to treat a Bubo which does come to abscess. When it is evident that a Bubo is coming to abscess, and that it cannot be prevented—do nothing more medicinally, wait the event—let the patient enjoy pure air—let his food be nutritive, and his habits such as should be practised for perfect health—let him be rendered fitting to support the process, and to be in a future condition for the kindly action of mercury—let the Bubo remain as long as it can without being opened—let as much of the indurated parts as possible be dissolved, and the Bubo will plainly demonstrate the choice of time for a discharge of the matter by its natural approach to effect it. The assistance which is necessary for effecting this will be by the application of a cold poultice made of bread and lime water to the part, and wait the continued discharge of matter for a week, when all fever will probably be at an end. Then is the time for considering what mercury has been given, if any has, and what must be still given: by referring to the effect which a former application of it had, one may better ascertain any further relative necessity for it. In the course of a

* John Hunter has hardly noticed the symptomatic fever, the hot and burning fits, and the profuse perspiration, in this stage of Bubo.

fortnight the sore will be clean, and will take on a disposition to heal. Mercury, when it has done so much, should still be continued, unless it be observed that the process of healing hesitates, and which it will always do after it has looked most promising, where mercury has been pursued too far. If such hesitation take place, mercury should be directly left off, and the constitution left freely to recover itself. Change of air and simple diet will then finish the cure. But if mercury be gone on with when the face of the Bubo indicates a change for the worse, every symptom will be aggravated, and the promising sore, from an almost certain appearance of immediate healing, will be degenerated into a bad conditioned ulcer, which no topical application can serve; but which, whenever it is the case, must be cured by abstaining from mercury, and by giving the constitution an exacted time for the bad state of it to be repaired: and that sometimes will be found to be an intricate and arduous task, a tedious and dangerous process, because mercury has been so injudiciously continued.

In my opinion, when such is the case, there is nothing so effectual as pure air, milk diet, warm bath occasionally, bark, and nitric acid, for obtaining a cure. Opium should be given freely at first, and gradually left off in the end, seeing that it suspends the stimulus which is irritating the habit, and that the effect from it can only be supported by repeated doses: but if it were continued longer than the sti-

mulus which it was intended to suspend, it would be found to act prejudicially, and the constitution would of course be alike injured from that cause; or the recovery of it to a pure state would at least be protracted. When a constitution is heated or inflamed by irregular living, preparatory to any attempt of mercury, that should be corrected; and time should be allowed for the patient to recover from the effects of a symptomatic fever, during a Bubo coming to abscess—for if mercury be given instantly, the febrile pulse will never subside, nor will the fever abate; as the constitution will be additionally oppressed by the influence of the mercurial stimulus being immediately joined to the venereal.

It is a fact of the first importance to be attended to, and which proves more than volumes written upon the subject can—that when a bad conditioned Bubo appears early after the opening of an abscess, either mercury has been previously given for dissolving it—or the constitution of the patient has been otherwise harassed. But in the former instance the case will be still more plain—in that, the indicative cause of a change in the condition of a Bubo points out itself: for if the patient has gone on regularly, and if the sore has been almost brought to an immediate point of healing—what other proximate cause can there be for its degeneracy but that mercurial system which has been injuriously persisted in?

Topical applications are next to be considered, both as they will be necessary for healing a Bubo, as

well as for a Bubo badly conditioned. I am never in dread of an induration from Bubo, knowing that every stimulus will support it, and knowing that it will be gradually dissolved, whenever the stimulus from any cause which is the support of it, be completely removed. It will be by the operation of time alone that an indurated Bubo will subside. When a Bubo has begun to granulate and look clean, the poultice which was continued, from the time it was opened, may be then omitted, and the dressing may be dry lint; or, if occasion should indicate it, vitriolated lint in the mildest form. And if the patient take exercise, a linen roller, of some length, and well applied, will essentially assist the cure.

With respect to a bad-conditioned Bubo, nothing can be done, or expected to be done effectually for it, until its amendment evidently arises out of a correspondent amendment in the constitution. But a progress of it may be checked, and a treatment of it topically may be conducive for guarding and repairing any destruction of parts, and for advancing them to a degree of healing.

The following is a form of treatment lately and successfully adopted.

CASE I.

A young Englishman arrived from Portugal, and in a few days applied to me. He had a Bubo

in the groin in a sphacelating state. The ulceration was cavernous, and of the extent of the palm of the hand, the edges plainly indicative of an ulcer continuing to spread. His report was full of incongruities ; I could make out that he had a Chancre, but it healed before any body besides saw it; that shortly after he had a swelling in the groin, and that he had done ever since what his Surgeon directed ; that his mouth had never been sore, nor had he in any instance during his complaint any thing like a sore mouth. He appeared feverish, and very much emaciated. He said he had a bad voyage, was seasick almost all the passage, and that all the fresh meat on board had been consumed early in the voyage.

Upon the closest inspection, I did not find any other symptom. He was put upon a generous diet, and took bark with nitric acid, in full doses. The ulcer was dressed after the following manner :

R. Decoct. Hord. perlat. ℥iv.

Acid. Nitros. Gutt. vj.

Pulv. Opii gr. xv.

Oxymel æruginis ℥ij.

Misce.

A large dossil of lint wet with this was laid on the ulcer, and changed every two hours. To facilitate the dressing, a piece of silk oilskin attached by slips of sticking-plaister kept the dressing on, and by taking off one of the slips, the ulcer could be then dressed without the smallest inconvenience. The

patient took no mercury. The ulcer healed in the course of three months without a change of medicine for the first two months, when all occasion for it then had ceased.

CASE II.

CHANCRE AND BUBO ON A SCROPHULOUS CONSTITUTION.

A gentleman, under thirty years of age, had a Chancre proved from his relation of his case, which, at the end of a fortnight, obliged him to keep at home. A swelling of the groin followed closely the assumed malignity of the Chancre. The constitution of this gentleman was of an extraordinary kind. He had for some time lost his front teeth, and so had his brother, who since died of a scrophulous consumption. The Chancre became formidable from its approximation to the Urethra. More mercury was given from that cause, and in all ways, than I have ever seen, not excepting fumigation. No sign of mercurial action could be elicited: yet there appeared no warrant for discontinuing mercury, as the threatening state of the Penis altogether, and of the Chancre, was not worse at the end of a month than it was when it was first begun with: and I am of opinion that without mercury having been thus pressed forward, he would have suffered the loss of the Penis at least. In this case the constitution was

in fault. Mercury did not seemingly act, 'tis true ; no more effect from it even in the largest portions could be elicited from week to week, than in general cases is produced from day to day. He was not unattended, for he had two Physicians, and two, sometimes three, Surgeons. At the end of two months, mercury was withdrawn for an entire month. The Chancre and Bubo grew worse. Mercury was again given, and in another month the loss from the month's absence of mercury was repaired. By persisting with mercury, the Chancre, in the course of five months from the beginning of the case, healed, and the Bubo, by following the same course, healed also in seven months. The state of the patient, when mercury was withdrawn after the Bubo healed, was that of weakness. His stools were bloody, and his legs swollen about the ancles, and he complained much of the tenderness of the Tendo Achillis. But it ought to be recorded as a part of this uncommon case, that from that time the patient has never enjoyed a better state of health.

CASE III.

A COMPLICATED CASE.

A gentleman had two Chancres, extremely simple, situated one upon the frænum, and the other at a little distance from it. By dry lint alone, and by mercury given in the form of ointment, these

Chancres proceeded to heal without much spreading, and with no other circumstance worth remarking. But before they had entirely healed, a pimple was discovered on the external part of the prepuce, upon that part which is facing the abdomen. This pimple was at first only red, and the part around it was thickened. I was rather surprised at the sight: by mercury having been previously given for the other Chancres, the appearance of this was changed from the form in which it would have been seen to be truly venereal, into another form; it was the appearance of a Chancre influenced by mercury, but not destroyed, and deprived of venereal action. It was the disguise of the Chancre which made me hesitate, and therefore I stopped a continuance in mercury, for two reasons; because I saw that the constitution was a good one, and because I doubted whether this was a Chancre or not. The ascertaining that fact, in the first instance, I judged to be of more importance than securing the cure of the first Chancres. By discontinuing mercury, the suspected Chancre appeared evidently a true one; and then by returning to mercury, the two first Chancres healed; but this last stood out for a longer time, discharging ichor, and reddening around it. The gland of the groin began to swell, and was rather painful. I continued mercury very freely for ten days, and found at the expiration of that time that a Bubo had come very forward, and that the fever had a considerable effect upon the health of the young gentleman. I

therefore dropped mercury, and did not see him for seven days after, when I found that the Bubo was ripe, and that the third Chancre was red, hard, but not ulcerous. I desired him that night to lay on a poultice—to take only some vitriolic acid three times a day, in cold-water, and to live in other respects in such a manner as was most conducive to health. I saw him the next time ten days after, when I found that his fever was gone, and that his spirits and appetite were mended. The ulcer had not as yet a clean appearance, but there was no hardness about it. I then began with mercurial frictions, which were continued on for near three weeks, until the sore was just upon the point of healing, and until the third Chancre upon the prepuce had for some days been quite healed. At this period the patient became drowsy, he had languid swoonings, his pulse was feverish, his eyes were reddened, and the Bubo was swelling. The little sore appeared glassy; I desired him to go once into the warm bath, to return to his vitriolic acid, and also to wash off and refrain from mercury. This he did, and soon got well, as soon as from reason it could be expected.—The leading points which claim our present attention in this case are, that the application of mercury, fairly and regularly administered with a view of dissolving the Bubo, failed: and that after the Bubo was opened, a continuance of mercury really produced that mercurial change in the sore when it was just about to heal, and which indicated a necessity for then discontinuing the application of it.

CASE IV.

A gentleman was at Plymouth when he first perceived a Chancre, for which he took mercury in the alterative manner. The Chancre soon after appeared to be healed, when he desisted. Five weeks afterwards, he came to town, and then I saw him. Upon examination, I found that the seat of the Chancre appeared red, felt hard, and upon pressing it discharged an ichor. I found that one of the glands of the groin was swelled, but was not tender. The gentleman was grown thin, and had febrile symptoms upon him, and I dare to say, that in a very short space of time he would have experienced constitutional symptoms. The inguinal intumescence, as I before observed, was free from pain, and had been stationary a long time, and I am of opinion that it would have continued in that state, if mercury had not been necessarily given. As this gentleman was going abroad at a stated time, I proceeded to the cure with mercurial inunction, and the Bubo as regularly, during the application of mercury, came forward to abscess. The Bubo, after coming to abscess, assumed a very kindly appearance in consequence of my desisting from mercury, upon finding that it was certain of coming to abscess. But as the gentleman was going abroad, and as the disease had been long upon him, I again returned to mercury, during the healing process of the Bubo,

and continued it for fifteen days. As soon as the effects of its action became evident, the Bubo again, although just healed, betrayed signs of fresh inflammation. I desisted then from mercury, and my patient went to sea; the inflammation subsiding, the Bubo healed kindly without mercury, and no other symptom afterwards appeared.

I attended a case during the last Summer of 1817, with a very ingenious young Surgeon, where a Bubo, just as it was upon the point of healing, betrayed the most malignant symptoms, and where the blame of it was clearly with the patient. This bad case continued during the winter. Every means which I have stated were tried. But his constitution became so depraved, that no method of treatment appeared to claim a preference. Wearied, at length, by ineffectual remedies, we recommended the patient to go in the country, and in process of time his ulcer healed.

CHAPTER XV.

ON THE CURE OF VENEREAL SYMPTOMS
FROM CONSTITUTIONAL INFECTION.

THE cure of every venereal symptom alike consists in a due action of mercury upon the constitution; therefore much more upon the subject cannot be necessary. Judicious discriminations in the use and abuse of mercury, can be somewhat farther observed upon: the constitution, independent of the disease, will exact them; and the disease, independent of the constitution, will also exact them. For there is a time when the disease must be protracted, for the sake of the constitution; and when the constitution, for the sake of its preservation, must necessarily get rid of pressing venereal symptoms. And although the disease and the cure of it are, upon a general scale, as familiar to almost every person as any two facts whatever, yet so difficult has it been ever found to form certain principles for eradicating the one by administering the other, that neither time nor practice have as yet established a given direct rule for successfully effecting the intention: because every case is dependent upon the constitution proper to every subject, and therefore every case has its own particularity. Without see-

ing the case—without trying the effect of mercury upon the constitution—and without looking to the event, it is impossible to pronounce in a decisive manner whether the disease be eradicated, when the symptoms of it are extinguished—or what will be the effect from mercury on a constitution before it has been tried.

From our present knowledge of the nature of the poison, and of the action of mercury, we are enabled to discriminate true venereal symptoms, to ascertain a judicious application of mercury, and to conduct the process of mercury, by observing its relative action upon the venereal symptoms and the constitution; we are enabled, "as far as the limitation of human power permits us, to know how long mercury ought to be continued, so as to eliminate the disease. But when *Lues Venerea* has been a long time associated with the constitution—when it has been assimilated with every part of it—when every fluid and solid may be said to be under the influence of the poison—no one single mercurial course can perfectly eliminate it, although mercury might have eliminated the evident symptoms of it. In such cases, mercury should be administered in a longer process—three or four months should be taken for the action of it, and the constitution should be, during that time, supported under this continual conflict.

When symptoms of the most obdurate infection have been completely done away, the patient should

be allowed time for recovery, and the return of symptoms should be carefully watched:—always recollecting, although the action of mercury might not as yet have completely eradicated the disease, that it is notwithstanding the only specific for obtaining that end—and that the default is never in mercury, but in the constitution, with which the disease is so completely interwoven. Although it would be imprudent to persevere in mercury long beyond the time of a disappearance of evident symptoms of the disease, so should it be reassumed whenever there be a palpable return of those symptoms. I cannot put this argument in a more strong situation, than by comparing it to the case of a fire. When a house is on fire, the engines are brought: from these engines are poured as much water as the firemen presume will extinguish the fire: but although the firemen are induced to desist from pouring on more water, because it appears to them that the fire is already extinct, and which is the only way they have for directing them how to act, yet they do not directly shut up the plug-holes, nor withdraw the engines from the spot; but they wait to see if the fire break out afresh, and if it should, they do not then infer that water is inadequate to the purpose of extinction of fire.

It appears the most improbable of all expectations, that for the most trifling symptom of constitutional infection, the alterative plan of curing the disease, whilst the patient associates in company and goes

about in the open air, can be depended upon. It appears, that the action of mercury, where the alterative method has been effectually practised, will fail of its due effect—will be acting upon a debilitated constitution, when afterwards the true method of applying it is adopted, immediately upon a default of the alterative method.—It appears, that the alterative method is only winked at against conviction by those who permit it, and by those who are conversant in practice.—It appears, that if there were no better prospect from future resources—no alternative after the alterative method has failed—that the disease could never be cured.—It appears, therefore, that there are but two rules to be constantly observed in the application of mercury:—That the quantity of mercury which is required to perform a perfect cure, may act upon the constitution with a force that is necessary for accomplishing the desired effect—and that the quantity of mercury which is necessary may continue to act with an equal force, and may support that change which it produces for a sufficient length of time, until there be no part ineffectually acted upon—until no atom of infection be latent behind, by which a relapse of symptoms may follow. They offend against the former rule who administer mercury in too small doses, or at too great a distance of time, by which there is not a sufficient quantity conveyed into the habit for producing the requisite effect; and they offend against the latter, who discontinue too soon the application

of mercury, by which the change which is produced in the habit, is not supported for a sufficient length of time for eradicating the disease. In the alterative method of administering mercury—it is common to offend against both rules: hence it appears, that this method so often fails of success; for the violence of the symptoms being mitigated, the disease only remits, the infecting principle is not destroyed, nor consequently the disease extirpated: and hence proceed those anomalous cases, where the symptoms vanishing, the patient indulges the hope that every thing has succeeded to his wishes; but, after a short respite, he relapses into a condition of disease worse than ever.

From all that has been said, it will naturally be concluded, that the chief discriminations which the cure of every constitutional venereal symptom exacts, are those which arise out of the obstinacy of the symptoms—out of the various parts diseased—out of the length of time that the disease has remained—and out of the immediate deranged health of the patient.

CURE OF PRIMARY SYMPTOMS OF AN INFECTED CONSTITUTION.

The primary symptoms of an infected constitution generally appear on the softer parts, as I before remarked, shewing themselves by eruptions on the

skin, and ulcers on the tonsils. These are also generally the result of preceding febrile symptoms, and evidently denote the infection of the whole of the constitution. For the cure of the eruption, mercury should be introduced in the form of unction, as evidently preferable in every respect; and the perfect cure of the eruptions, as well as the prevention of future symptoms, exact a strict attention to every minute circumstance, as if the symptoms had remained longer and were more multiplied. By increasing the proportion of mercury according to the action of it regularly every day—by bringing on a mercurial change—in the course of three weeks the symptoms of the disease will begin to vanish—and by continuing mercury for the support of this change three weeks longer—the whole of the eruptions will be effectually removed—leaving the skin smooth, over which a new cuticle will soon be formed; by so doing the hardness of eruptions in harder parts, and clefts in the hands and feet, will also become smooth and healing. These symptoms of the disease are the most pleasant of all venereal cases to be cured. The contest will be fair between the mercurial action and the venereal: and mercury strictly and successively administered, will gradually extinguish the venereal action, without interference of any topical applications.

There will, in these recent constitutional affections, be commonly some trace of disease on the original local part that was first the seat of the dis-

ease, and previous to its being absorbed into the habit—there will be either a hardness upon the seat of the Chancre, a weeping ulcer, warts on the part, a thickening of the prepuce as if it were horny, or an enlargement of the inguinal glands, or an ulcerated Bubo, or an ulcer any where else. All these commemorative signs of prevailing constitutional disease will surrender their virulent marks as the eruptions on the skin disappear. The dulness of the patient, the loss of appetite, the emaciation from fever, will also be totally evanescent. And although he may appear wasted through mercury and regimen, the first fair day of fresh air will shew a change in the whole of his complexion—the constitution being freed from the agency of both the poisons. All this is procured through that specific power of one medicine alone, which the single or joint power of the whole materia medica could not besides effect.

But although mercury possesses a sole power of eradicating the disease, yet that cannot be obtained without its action being continued for some time after the venereal symptoms are eliminated—not even on a constitution in the most favourable state for its action.

CASE I.

An Officer, who had been quartered in Birmingham, applied to me in the Spring of the year. He

had febrile symptoms, was wasting, his appetite failed, and a strong constitution was impairing. Eruptions were apparent on his forehead, shoulders, arms, and thighs. A hardness remained on the part where the Chancre had been, and the glands of the groins were knotty. The Chancre had healed about six weeks. He had been taking mercurial pills from time to time, but had never confined himself. He began with mercurial frictions; a drachm and a half every night was rubbed on the thighs, and in the course of ten days his mouth became affected. The same proportion was continued three days longer, when his mouth became so affected, that he only repeated his mercurial frictions every other night. During the process, the eruptions gradually disappeared, and at the expiration of three weeks, the Officer being impatient to return, and tiring of his mercurial state, left the mercurial symptoms to subside, and joined his party at Birmingham, more satisfied with his condition than I was. In a short time after, he went to the West Indies, and after remaining there four months, returned with the following symptoms. Eruptions more generally over his body, pains in his cylindrical bones, nodes on his forehead, and a constitution more wasting than by the former attack. He very cheerfully submitted to a second process. He gradually continued mercurial frictions for seven weeks, and owing to a kindly action in mercury upon a constitution radically good, he got completely well.

Had this patient, in the first instance, but persevered a week longer in the use of mercury, he would have prevented a return of the disease, and second sufferings of a cure. The folly of being too impatient, and desisting from mercury too soon, is as great in a patient, as in him, who, in order to arrive at a place of destination, sets off to perform his journey—turns back before he has reached it—and is afterwards forced to set off again.

CASE II.

A young person had a sore under the frœnum, which was, from concomitant circumstances, considered and treated as venereal. It was brought to a state of healing, by taking pilulæ hydrargyri five grains every day. As soon as he thought the sore was healed, he took no more pills, and he was not seen again till three months after his voluntary departure. He then had a symptomatic fever, a sore throat, eruptions on his body, and a quickened pulse. The seat of the original sore under the frœnum was inspected, and there was a considerable induration, as hard as a pea, and as large. He did not choose to retire to his chamber, and therefore he was directed to take five grains of pilulæ hydrargyri night and morning, and also to rub in one drachm of unguentum hydrargyri every night. This made his mouth passing sore, and by his going on with this for a month, all his symptoms were gone,

but the callous tumour on the frœnum ; of this there was but a very little change, certainly rather less than more. He again took his departure, and appeared before me again after the fifth month. All the symptoms before related were on him now, and in an exacerbated degree. His constitution seemed to have suffered more in the conflict than it had in the former instance. He now submitted to remain in his chamber, and the same quantity of mercury, after the same manner, regularly administered a whole month, removed all the symptoms, but the callosity under the frœnum. This circumstance convinced me of two things ; first, if it was meant to go on with mercury ever so long, it was most probable, this callosity would not give way. Second, if it remained, all the symptoms would, to a certainty, return again at some remote period ; or, perhaps, some other symptoms of an infected constitution. The callosity was dissected away, and the mercury continued on a fortnight longer : the part healed readily, and from the time that has since passed, and from the appearance of the young person, there seems to be no probability of the symptoms again recurring. My opinion of this case is, that without the removal of this callosity by the knife, mercury in any quantity would not have effectually cured it.

ULCERS ON THE TONSILS, &c.

The next symptoms which I mean to treat upon, are venereal ulcers on the tonsils, and which, if neglected, will also persist in destroying the uvula. Sometimes both tonsils are affected, and sometimes eruptions on the skin are concomitant with ulcers on the tonsils. An ulcer, thus situated, first attracted the attention to fumigation, but which is now, in my opinion, with great justice exploded. My wish is, never to disguise any venereal symptom—never to apply any topical remedy to it, if I can avoid it. I wish to be apprised of the worst of the venereal action, and the whole of it—and I wish to see the pure action of mercury take effect. For that reason, instead of topical applications, I exact from the patient to submit to rule—that the full force of mercury may not be interrupted, neither through omission nor through intemperance. By leaving the ulcer to the event of the action of mercury solely, I see when the venereal part of the disease ends, by the ulcer healing under the kindly action of mercury; and having once seen the sanative power of the action of mercury, I know that if the ulcer—after it has commenced to become clean, and after it has almost been healed—again shews a disposition to increase, inflame, and become a spreading ulcer—that it then is no longer a venereal ulcer.

Without leaving the parts to the prevailing action of the two poisons, this could not be known: nor without the first effects from mercury, which were kindly, being succeeded by second effects, which were marks of the abuse in the power of it, I should neither know when to distinguish the predominant action of the one from the other, nor how to treat that ulcer—where from no peculiarity in the proper appearance of it, nothing could be by it discovered. Therefore, the whole of such a case should rest upon the prudent administration of mercury, and that should be conducted as I have already pointed out. All those badly-conditioned ulcers, which clearly arise out of a degenerated state of the constitution, in consequence of mercury being pressed too far, should be considered as requiring an amendment of the habit by nitric acid, bark, air, pure diet, and regularity in every respect. Bark in decoction, or barley water, with a little tincture of myrrh, will be a proper gargle for washing the mouth.

On the tonsils there will be sometimes a horny induration—somewhat similar to that on the prepuce—and the uvula also will be indurated and rigid when an ulceration on it is stopped by mercury. But when mercury has not been completely gone on with, and when restringent washes to the parts have been improperly tampered with—to cure these it is necessary to pursue mercury farther: for, without the glands be restored to their original secreting character—without the symptoms of the venereal

infection, which are evident, be completely removed—it must be concluded, a priori, that the constitution cannot be said to be freed from that which, through it, produced its disease. The tonsils will also be found to be enlarged sometimes during the action of mercury; but when that action is withdrawn, these enlargements will go down, or at least if they should not, I know no art that will conduce to promote their subsiding.

CASE.

A Gentleman applied to me with the following symptoms. A thickened prepuce, a foul discharge from behind the glans, a hectic heat, an emaciated aspect, and tonsils uncommonly enlarged and irregularly ulcerated. He had experienced a soreness in his throat for more than two months. He began with mercurial frictions, in larger proportions than I wished, two drachms every night. As my patient, on account of public business, was impatient, this was continued for ten days, but producing not the fair effect, three drachms were repeated for ten days more. From this as little of the action of mercury was evident: that action from it which was apparent, being deleterious instead of kindly—for his mouth was unaffected, and his head was giddy, and he was disposed to swoonings. After a continued application of mercury ten days longer, these symp-

toms from it were more increased, but the venereal symptoms were almost evanescent. The prepuce was quite thin, the discharge behind it had ceased, the tonsils looked clean, but their enlargement had not abated. Desisting then from mercury, my patient went a little way out of town, took bark, nitric acid, and did every-thing for the restoration of his health.* In a short time afterwards, the tonsils assumed again their former state of ulceration, but the health of the patient was mended. Mercury again was administered, in almost as full proportions: in the beginning it acted kindly, and affected the mouth, which deterred me from persevering with repetitions of such proportions. It was then administered in half proportions, by which the salivary symptoms declined, and could never be brought on again, although the mercurial frictions were again increased to as full proportions as in the former process. After pursuing this mercurial system for four weeks, during this second course, the tonsils had for some time appeared very clean; but their enlargement still remained, and every other venereal symptom had been long extinguished. The patient was very much debilitated by the action of mercury, and by confinement; I therefore advised him to desist from mercury—to pass his time quietly by the sea side—and to wait the operation of pure air, regular living, and sea bathing. This method then succeeded, and he continues well, at least to this time; but I know not whether the enlargement

of the tonsils has yet subsided, as the case was evidently engrafted on a scrophulous habit.

In the treatment of all venereal symptoms, on every part of an infected constitution, there are two material discriminations: the one is, if the treatment be commenced for the cure of them, when the symptoms are recent; and the other is, if the treatment be commenced for the cure of them, when the symptoms have gone on, and when, through that long progress of them, other parts of other anatomical descriptions situated in contiguity to them, have fallen in for their portion of venereal destruction. The treatment of the first will require less time, and will be more simple; and the same treatment for symptoms at their early appearance, will be almost equally effectual, on whatever parts such symptoms appear.

Then supposing, in the first case, that there is an indication of venereal symptoms on any of the parts which are most in common subject to venereal symptoms—if upon the first discovery of them, mercury be immediately and effectually given, the progress of such symptoms will be stopped, and the parts on which the symptoms appeared, will need no other assistance for their restoration than the bare constitutional action of mercury; whereas, if the disease be permitted to continue its action on these parts, they will be found to be in such state of disease as is peculiar to the anatomical nature of them: for although mercury will be in this instance more ne-

cessary, and a longer time must be exacted for the cure of the protracted disease—and although mercury will cure Lues Venerea which was the cause of the disease on these parts, yet mercury will not restore them to a sound state, nor will the parts themselves become restored to a sound state, not after Lues Venerea is eradicated, nor after mercury is withdrawn. Each particular part will then exact particular chirurgical treatment for the cure of it; and without chirurgical aid, diseases on particular parts will go on, merely as diseases of parts, independent of any constitutional cause whatever.

Having premised this—I shall proceed to state all those incipient symptoms which I deem to be curable, without any other interposition than mercury insinuated into the habit; or at least very little conjunctive assistance will be required for this stage of the cure, in addition to mercury; and what that assistance is to be, must depend upon the nature of the part. An incipient ulcer on the palate, œsophagus, uvula, tonsils, nasal or auricular membrane, a commencing opthelmy, ulcers on the forehead, a thickening of the spermatic chord, enlargement of the testicle, ulcers on the scrotum, sanies discharge from the anus, rhagades or loose fungi about the anus—all these require the full and decisive operation of mercury. And as the progress of the disease on many of these parts would be fraught with not only local mischief, but would also bring on diseases on contiguous parts, so should the utmost attention

be exacted, that the mercury be always liberally applied, and that the state of the patient be always such as will be most conducive to a decisive operation of it. Two drachms should be rubbed in of the mercurial ointment the first week daily, and three in the succeeding week, until evident signs of its action appear, when the quantity may be reduced, so that the action of it may be supported for ten days after the symptoms are become evanescent. In this instance, when the promised success is fully answered—by leaving the symptoms to speak for themselves, by leaving them untouched, the action of mercury upon them will be better discovered; and at any rate, when that action is as it ought to be, they will need no other assistance for the effectual cure of them.

Supposing, that if any part derived any benefit, on the first commencement of disease, from topical applications, it must be a commencing venereal opthelmy; but what can stop its progress but the action of mercury*? But if—in consequence of the rapid progress of the disease, and before the action of mercury could take effect when it has been begun with as early even as the nature of the disease could be ascertained—the symptoms have notwithstanding spread, and other parts are involved in disease, I shall state in that particular, which I term the second mode of treatment, what chirurgical assist-

* Vide Professor Schmidt, Becre and Travers.

ance must be exacted, independent of the action of mercury.

Ulcers on the palate—within the nose—within the ears—on the head or forehead—and on the internal parts of the upper and lower jaws, may so proceed, that not only the softer parts will be destroyed, but also the covering of the bones will be destroyed, and the bones adjacent to the ulcers will become caries: these bones once becoming caries, will naturally protract the disease, and will never recover from disease; caries must have time to be exfoliated and separated from the sounder parts of the same bones; or the bone altogether, if the whole be entirely a caries, will come away. A caries bone is to be considered as a dead part, as an extraneous part, as obnoxious to the living principle of remaining sound parts: it will, and must be separated, and time must be allowed for that process, after the disease which caused the caries be entirely done away.

In my observations—upon the cause which produces caries of bones, in the instance of a recent venereal constitutional infection, and upon the cause which produces caries of the cylindrical bones, and the epiphyses of them, in the instance of a long-continued and inveterate infection—there will be a most material and important distinction. The former bones become caries in consequence of the destruction of the softer parts about them; and the latter become caries in consequence of the venereal action within them. The former will be the effect

of bones being deprived of their covering, deprived of nutrition, deprived of attachment, and of being exposed to air—the latter will be the effect of a long and complete venereal action radically on them—will be the positive action of the poison—will be the process of months, and sometimes of years—cannot be effected but through exquisite pain—cannot be removed but by a long mercurial process—and cannot sometimes be removed but by parting with the diseased part through large and repeated exfoliations.

When softer parts are destroyed, and coverings of those bones are so also—when parts to which bones are annexed, are also destroyed, and the bones are exposed, either in part or wholly, whether the disease which produced this destruction were venereal or not—the bones would alike be caries. It is therefore incumbent, in this instance, to be particularly guarded in observing the action of mercury; and to know, that as soon as mercury has effected the intended purpose of extinguishing the venereal action, which it will do in almost the same space of time that it would have cured the symptoms, before they had gone on so far in destroying the parts—then the quantity which is administered should be gradually diminished, and the balance should be kept so even, that the patient may not fall into a contrary disease, from the preponderating action of the mercury, and in consequence of its being pushed too far. Before the whole of an exfoliation is complete, it will be some time; and dur-

ing which the constitution should be restored by air, diet, nitric acid and bark.

If the disease be in the mouth—the parts may be washed with barley-water, honey of roses, and tincture of myrrh ; and the nose and ears may be also syringed with the same fluid. After the venereal action has been destroyed, lamina of the bones of the cranium will sometimes be exfoliated from caries there : until the exfoliations are completed, none of the parts will heal, although, when the constitution is freed from the virulent cause, and when air, diet and strict temperance have been observed, the propensity for healing will be discovered to be very strong.

CASE.

A naval Officer, when at sea, was attacked with a venereal ulcer on his palate, and which continued to spread to so alarming a degree, that he was obliged to quit his station, and return to London. When I saw him, he appeared to be in the last stage of emaciation : the ulcer had destroyed the palate, had extended to the gums of the upper teeth, all of which were dropped out, and his voice (or rather noise) was so inarticulate that he could not be understood. This ulcer had made its appearance six months before, at which time the patient had been living freely : he told me, that at no time mercury

had done any good for his cure : but upon more strict inquiry I found, that at no time there had been any serious attempt for perfecting his cure by a regular or judicious administration of it. He had not laid by—his vitiated state of constitution had not been corrected—nor had he for two months taken a single grain of mercury. On his passage home he was attacked by continual boring pains in his cylindrical bones ; he had nodes on his elbows, and on one of his parietal bones there was a soft tumour, containing fluid. He had a violent cough, and his pains in the night were most tormenting. There were two objects to be most attended to in this case—the delicate state of the patient, and the disease with which he was possessed. It appeared to me, that if mercury were pressed too rapidly, the patient would sink ; I therefore put him upon milk diet, and began rubbing in three drachms of the ointment in a week. This system was persevered in for the first month, at the expiration of which time his health was better, the ulcer looked favourably, his pains were abated, and his nodes disappeared. During the next month, the proportions of mercury were increased to a drachm and a half every night, the patient using the warm bath to check the too rapid progress of its symptoms, which were strongly disposed to come on. At the expiration of this time, the whole of the venereal symptoms were evanescent ; but the sockets of the upper teeth were not exfoliated. It was to explain the following

points that I have given this case:—They are, that Lues Venerea, which was the cause of the ulcer, being at an end, proceeding farther with mercury would have been injurious—that the sooner the patient was restored to health, the more kindly would the exfoliation follow—that in a month after a very large exfoliation did follow — and that granulations soon covered the sound parts of the bone. The patient always applies lint to fill up the cavity in his palate, and by this he articulates very well, his upper lip being very long: and I question whether he may not, if he pleases, conceal the whole of this unfortunate calamity from the observation of his most particular acquaintance.

VENEREAL OPHTHALMY.

When a venereal ophthalmia has made any progress, the inflammatory symptoms may exact rather particular attention: at least, it will be expected that something topically should be gone on with during the mercurial process, and even after, for the injuries done to the component organs of the eye. Blisters, leeches to the temples, fomentations of warm water with opium dissolved in it, avoiding the light, and at the latter end of the cure, a vitriolic wash, or brandy and water, for the restoration of the distended vessels, should be applied *.

* Vide Professors Schmidt, Vienna; Beere, Sir William Adams, Travers, and Quarterly Journal of Foreign Medicine and Surgery, No. I. November 1818.

Whenever the lachrymal sac and the lachrymal duct suffer from venereal infection, the extinction of that infection may not procure their restoration to a sound state. The art of Surgery particular to that disease must be appealed to ; the fluids must either find their natural passage restored to them, or an artificial passage must be made for them. The same may be said also of the necessity of operations which are particular to the cure of a diseased state in other parts. A fistula in ano, from a venereal cause, will not be cured as a fistula when the virus is extinguished. But in all these cases mercury must be pursued, as I have before laid down, until an elimination of Lues Venerea be complete. The constitution, by being freed of the original cause, and of the mercurial process, will be then in a better state for any operation necessary for the cure of a disease particular to a part : and during that operation no other appeal need be made to mercury, unless appearances demonstrate the necessity, which can never be the case, when Lues Venerea is entirely eradicated : but rather bark and restoratives ought to be then administered, that the operation and cure may proceed with success.

CURE OF DISEASED TESTICLE.

The testicle will also require attention : if, from long Lues Venerea the testicle and the spermatic chord be indurated, if the vessels be no longer in a

state for vascular offices, no power can again restore the testicle to its original state, nor fit it for its former functions :—its extirpation or its remaining—its recovery or not, must be an object of local attention, when Lues Venerea, from a thorough action of mercury, is out of the question. In short, as the constitution becomes deeper and deeper infected with the disease, so will the disease betray itself on every part :—the lymphaticks will betray it, and the whole of the secretions will be vitiated.

CASE.

A Lieutenant of Marines had a gradual enlargement of testicle, the appearance of it was full as large as the largest hydrocele. It was hard and heavy, and constantly painful. He had a symptomatic fever, a hectic appearance, and perspirations at night. He was about 21 years of age. He said, that a twelvemonth since he had a Chancre, which soon healed. The seat of the Chancre was inspected, and there was a callosity remaining; the spermatic chord with some difficulty was felt, and found to be thickened. He said that the Surgeon at Portsmouth, from whence he came, who had attended him there, had recommended him to a Surgeon in London, — by name, but as the Surgeon at Portsmouth had hinted to him a probability that the testicle would be extirpated, he did not avail himself of that recommendation, but bethought on

his way to town of applying to some other. After having referred the case to the opinion of another Surgeon at my request, it was judged to be a case fit to be submitted to the trial of mercury.

This was the latter end of September, the patient got a recruiting order, and resided in the Tower. He solicited, that whatever we did for him not to confine him altogether, as in every other respect he would be faithful in following instruction. He took five grains of pilul. hydrargyri night and morning, and rubbed in half a drachm of unguentum hydrargyri every night. He took an emetic every Saturday night. The testicle was wrapped up in silken oil-skin, and suspended. The mercury just kept his mouth sore, the symptomatic fever left him in the first month. The callous seat of the Chancre was dissected away, for it would not subside. The mercury, leeches, the vomits, and the oil-skin, reduced this testicle to a sound state, and it was on New Year's day, that he desisted from all the means of cure, and returned to Portsmouth.

WARTS AND EXCRESCENCES.

Venereal warts and excrescences will also exact some chirurgical treatment: but that should not be put into practice until the latter end of a mercurial course, when mercury be fully acting. I attended very lately a case of this nature, and as the circumstances of it are rather singular, I will state them.

CASE.

The patient had the following appearances: an emaciated sickly countenance—weak eyes—hissing noise in his ears—a node just perceptible on his forehead—and a large fimbriated excrescence around the anus, discharging a sanies, and obliging the patient to straddle in his walking. A Surgeon in the City pronounced the case to be venereal, and my opinion was solicited to make his the more decisive. I did not hesitate to declare the same. The cure commenced by confining the patient to his chamber, and by mercurial frictions of a drachm and half every night: the mercurial action was remarkably kindly, and at the expiration of three weeks, by a continuance of the mercurial frictions, the fimbriated excrescence began to slough away: but as it held fast at the root, and as it grew out upon separate stems, four or five distinct ligatures were then passed around them at separate times, and the whole soon sloughed away. Mercurial frictions were continued for a fortnight longer, and the patient got well. When I first saw this patient, I asked him how long it was since he had a venereal complaint? He replied, not for seven years. I asked him if he had knowingly taken any mercury during the last seven years for any other cause? He replied that he believed he had for a sore leg. The question is—when this disease was contracted?

It must have been contracted seven years—or the virus must have since been absorbed in consequence of an infectious connection, without leaving any local impression. I believe that the present symptoms were in consequence of the known disease seven years ago—and I am induced to think so, because, if the virus had since been absorbed without leaving any local impression, the probability is, that the first constitutional symptoms would have been more notorious and rapid—or that they would have been in the form of ulcers on the tonsils, or eruptions on the skin. Or he might have contracted the disease during his taking mercury for his sore leg, and by that means have interrupted the regular progress of the first symptom's natural to the disease. For the removal of those soft warty excrescences which are found behind the prepuce, and upon the glans penis, sabine powder sprinkled upon them will frequently succeed, and more especially when they bleed at the touch: but when the stems of them are strong, and when they are grown hard, nothing but ligature will extirpate them. As they are generally produced by a redundant vitiated secretion, so should the parts be frequently washed with a weak solution of vitriol, to prevent their growth.

A variety of other parts, which may not be within my recollection to enumerate, will require the assistance of Surgery for the reparation of them, after the disease which was the cause be already eliminated; therefore, it is unnecessary to add to those

instances which I have already adduced. The proper surgical acts, which are proper to each particular part, must be so appropriated as I have already stated, and the consequences will equally evince that truth in the theory and the practice which first urged me to the recommendation of it.

There are but few more symptoms remaining, which are objects for present observation: for I consider subordinate symptoms always included in more important ones (as *minora* within *majora*); they will always yield to that judicious treatment which effects the cure of those in point of local consequences, and in point of local diseases more seriously deserving notice.

VENEREAL HECTIC.

A venereal hectic, attended with ulceration of the lungs, is an object for particular observation. This disease has not exacted all that attention which it ought; and this disease very often proves fatal, for want of discriminating it to be from a venereal cause. When it arises on a constitution not otherwise disposed to a consumptive diathesis, the effects of mercury will act upon it like a charm: but when it is in combination with a consumptive diathesis, although the venereal cause may be removed, and may be cured by mercury, yet there is nothing in that medicine which will conduce to correct a con-

stitutional disposition to pulmonic affections. But as this possibly cannot be known *a priori*, so must the case not remain unattempted to be cured by a trial of mercury, whenever there be indicative symptoms of a prevailing venereal infection, to lead us in forming such a judgment on the case. As this case may arise from the constitution being feeble, and it is an argument of a feeble constitution, so must these indicative venereal symptoms be not expected to be either numerous or very striking. Eruptions on the skin, and slighter symptoms of venereal infection which might have formerly fallen to the lot of the patient, will be sufficiently demonstrative of a cause for such a suspicion.*

I shall now point out the peculiar mode of treating a pulmonic venereal hectic: as in such cases the constitution is in a feeble state, the cure must not consist in exciting a rapid mercurial action: at the commencement mercury should be gradually introduced, and the action of it as gradually ascend to an *acmé*. The patient's health should be the first consideration, and mercurial action should be subordinate to that. He should enjoy pure air, milk diet, and he may take the *sarsa decoction*. After the process has been thus gone on with for a month or more, the indicative symptoms should be remarked, and if they shew a disposition to retire, and if the constitution gather strength, then may mercury be farther increased, in order to procure the more effectual action of it, and to produce that

change necessary for the perfect extinction of the venereal action out of the constitution.

**CURE OF GUMMATA ON THE APONEUROSIS OF
MUSCLES, NODES ON THE PERIOSTEUM, AND
CARIES OF BONES.**

When I was defining the symptoms of Lues Venerea, I observed, that before any eruptions, or any other first venereal constitutional symptoms appeared, there were, in this incipient state of infection, pains felt on various tendinous parts, particularly on the shoulders, the arms, the loins, and the hips; and I farther observed, that these pains were not permanently fixed, but that they were merely erratic, and that they were foreboders or precursors to venereal eruptions on the skin, and to other incipient venereal symptoms, and that these pains subsided, as soon as such eruptions, or such other incipient venereal symptoms advanced and became critical.

These are not the pains which I now shall consider, as they are not produced from the same cause, namely—not in consequence of the action of venereal foreign virus strongly impressed, and that for a series of time, on the tendons, the periosteum, and the substance of bones. This is that stage of the disease which I am now about to consider, and to cure that last and late action of virus on the con-

stitution—that which marks the power of it over the constitution as conclusive—that which, according to the descriptions of Vigo, of Fallopius, and most of the Authors up to the present æra, has been considered, as it actually is, the last and the most obdurate of venereal action. .

I also before observed—that as soon as the constitution was proved to be infected by the symptomatic fever, the erratic pains, and the subsequent appearance of venereal eruptions—a foundation for all future venereal symptoms was laid from that immediate point of time, and the constitutional infection then also commenced: but that the harder parts, by their being of that peculiar anatomical construction as not to admit of, and as not to yield to the force of venereal destructive power with so much facility as the softer parts do, consequently could not betray that destructive venereal power over them so soon as the softer parts. The peculiarity in the construction of these parts anatomically prove, that the disease must be later in betraying symptoms of venereal action on them, than on softer parts. They are composed of vessels far more minute and compact, and very little sanguineous circulation is discovered to pervade them: the living principle of them is supported by fluids which are circulated through minuter vessels than where sanguineous globules can find a circulation: and so rigid and so inadequate are tendinous parts to dissection, that it is effected with the utmost difficulty,

and attended with excruciating torture whenever such distention or diseased action on them is provoked even by any other first cause than venereal. Diseased action on them, either by gout or rheumatism, will produce all those painful effects from over distention and inflammation.

It therefore is evident, that any other diseased action on these parts will excite symptoms similar to the venereal ; but there is always this difference between them, that the venereal action will never be discontinued, without mercury ; it being produced by a foreign poison and an endless cause, whereas other causes and other diseases may decline and pass away of themselves. Nocturnal pains, from every cause, originate by the pressure of circulation, in consequence of additional warmth from the heat of the bed. The venereal cause produces the most intense and the most constant pains, and these will continue : the common irritation on these parts from gout or rheumatism may decline, and the nocturnal pains may subside ; but the specific irritation produced by foreign virus, will never be discontinued ; and therefore, as a poison,

“ *Vires acquirit eundo.*”

Having renewed these recollections, I shall proceed to farther remarks, by saying—that the time of the appearance of these symptoms is very uncertain—and that the uncertainty generally is in consequence of mercury being given, but not effectually,

for eradicating the disease. That the rudiments of the action of Lues Venerea commence on these parts, with the rudiments of the same action on softer parts, is proved by the following observations: First, that when there is no interference of mercury for curing or retarding the venereal action, these symptoms, although last, will immediately succeed to symptoms on softer parts, as it was evident in the case that I gave, of the lady who was attended by Mr. Pott, and when no mercury had been given, until these symptoms had actually appeared: and secondly, I prove it more familiarly by stating that which is constantly the case when these pains and these affections do come on? and thus it is—

Enough of mercury has been administered on a subject, for the cure of any of the constitutional symptoms which were apparent on the softer parts; and these symptoms, in consequence of that mercurial action, ample for that purpose, will disappear, and the patient is concluded to be perfectly well, and free of infection. At this period of time, no symptoms of the action of the disease, from the same infection, will be apparent on any tendon, periosteum, or bone;—the virus on the harder parts having not proceeded so far as to excite evident effects of commencing destruction, but the rudiments of the disease on these parts are commenced, by the virus having pervaded the circulation. The action of mercury, when it cured the venereal symptoms on the softer parts, pervaded

also the circulation through the harder parts ; but by its action being discontinued too soon, the venereal action on the latter is not eliminated, but retarded for a time, in consequence of which it in time again recovers from its crippled power, and indiscriminately returns at earlier or at remoter periods. The rudiments of its action therefore are laid long before that action may be apparent ; for so uncertain is the time, in the appearance of symptoms on the harder parts, that it may be one, two, three, or more years, before the venereal effects are perceived. Nothing can more clearly prove, that the rudiments of the infection commence on harder parts as soon as on softer parts, and that the difficulty in removing it is greater than that in removing the infection on softer parts—than the following

CASE.

A patient, as soon as a spreading Chancre healed, became rebellious to a continuance of mercury. He washed it off, and retired into the country, it being in the Summer months, and there remained till October. On his return to town, he applied to a Physician, having at that time nocturnal pains in his arms, shoulders, hips, and also enlargements on his elbows, and on his forehead.

If this patient had not, from necessity, during the protracted state of his spreading Chancre, continued

the application of mercury for a longer time than usual, eruptions on the skin, or ulcers on the tonsils, would have, in their common order, appeared. But these primary symptoms of constitutional infection were baulked from that cause. There was enough of the action of mercury produced for the prevention of them—but not enough for eliminating the rudiments of infection already commenced on the harder parts.

The Physician attended him for some time—he gave him hemlock, but that not succeeding, he gave him mercurial pills. The pains became relieved by mercury, but the disease was not removed. When the patient was at ease, he discontinued the pills; and as often as the pains returned, he returned to the pills again. By this, he kept the disease at a stand, until the next Summer advanced, when he went to the sea side, and bathed in the warm bath and in the open sea. In the Autumn he returned to town, with all the former symptoms: but by then submitting, from conviction, to a regular process of mercury during seven weeks, he got completely well.

For the effectual cure of gummata, nodes, and carious bones, there will be some necessary discriminations.—These consist in considering the various stages of the disease, the duration of it, and the effects produced upon the parts by mercury: these consist also in the state of the parts, when by mercury the cause is removed, but the diseased condi-

tion of parts still remain. As long as the disease has spared enough of the living principle for supporting the parts, the venereal part of the case is curable by mercury ; but when the disease has destroyed the whole substance of a bone, or the epiphyses of it, neither can be again restored. That which is once a caries will be always a caries ; as in the instance which I before stated, and from the same necessity in the nature of things. But it must be observed, that so strong is the resistance of these parts against the action of the disease, that it will be found acting on them a long time, and yet by mercury the parts may and can be again restored to a sound state. This is that stage of the cure, which I shall first commence to treat on.

At the commencement of the cure for at least the first six weeks, the mode to be adopted should be an intermediate one between the alterative and the decisive and more effectual mode : mercury should be gradually insinuated for six weeks, and the warm bath should be used twice a week. The patient should keep within doors, but not to his chamber : and for the ensuing six weeks, the process of mercury should be so advanced, and so supported, as to keep up constantly the mercurial change, and this should be continued almost a month after the venereal symptoms have been eliminated. The patient may take an opiate every night, and if he pleases, he may drink decoction of sarsa through the day. By this method, many of these venereal attacks

have been cured, purely owing to the action of mercury being decisive, and owing to its being continued long enough for procuring all its effect. All the cases, the symptoms of which yield to mercury, may thus be cured; and although they may return again and again, by mercury not being sufficiently persevered in, yet it must not be forgotten, that since mercury has once relieved the parts from the venereal action, it is the strongest presumptive proof that the case is within the pale of a cure through it.

It has been a question—what local applications are necessary to nodes on the periosteum, and to enlarged and diseased bones?—The practice has been with some to make an incision through the node—and the practice has been with others to lay a caustic on the part. When the skin is sound—or when it is discoloured—or when it is a little broken—or when there is even an ulcer on the part affected—provided that these appearances PRECEDE any use or action even of mercury, they should be suffered to remain as they are, until the evident effects of mercury, through the constitutional action, be observed upon them; and it will be found very often that these threatened operations will be unnecessary. The skin, from modern observation, is proved to be more and more an object for preservation, and he who divides it, when it might be dispensed with, does more harm than good, or than he can often after repair:—that labourer who can pull down St.

Paul's, may not be able to build an hovel. If an incision be made when the integument be whole, the deceased part under it will not suffer it again to heal—if caustic be laid on, if the periosteum be destroyed, and the surface of the bone be exposed—whether that bone were a caries or not before, it will most assuredly become so, and most assuredly an exfoliation from it will take place. But if, after a complete mercurial process, the integuments inflame, or ulcerate, there is then a presumption for concluding that the bone is in part caries—that the living principle of the remaining part is excluding it—and that it is an obnoxious substance foreign to life, about to be separated—and that by the best means it ought to be separated from the living part, before that part can become sound, and before the integuments will again be healed. When such is the case—I cannot object to the application of caustic—not but I am convinced, that in the end the integuments would continue to give way, until such time as the extraneous part be completely separated.

I shall lastly observe—that although Lues Venerea may be eliminated in all its stages by mercury; yet there are frequently instances from mistakes in cases, where the bones on various parts of the human body will be in that state of confirmed disease, from long venereal action on them, and where such will be the destructive effects, that they cannot be expected to be restored, neither by mercury, nor by any additional art that may be annexed

to it. It seldom happens, that where the living principle in one bone is so destroyed that there be not enough, if there be any, of the sound part remaining to give it support, and by which, it in consequence gives way—I say it seldom happens that the disease is confined to that bone in particular. Others will be found advancing in disease from the same cause. Bones not suspected will be sometimes forward in disease: this cannot be more strongly evinced, than in that case which is related by Mr. John Howard, of a girl whose chin dropped suddenly on her breast, and who expired, whilst she was under a salivation in St. Bartholomew's Hospital, and whilst her cure was in consequence looked for. It appeared, on examination, that one of the vertebræ of her neck was a caries—and had thus suddenly given way.

ON THE CURE OF VENEREAL INFECTION IN NEW-BORN INFANTS.

In addition to the case I gave of a child born thus infected by the father, in a part of this work which treated on Venereal Symptoms of an infected constitution, I should not do a duty to my own judgment, if I omitted to add a body of evidence in confirmation of the fact which will not be disputed.

Paul Augustin Olivier Mahon*, alive in the year

* PAUL AUGUSTIN OLIVIER MAHON was born at Chartres, the 6th of April 1752. He practised physic at Paris, was a

1800, chief physician at the Hospital du Vaugirard for venereal patients, has written all that can be collected upon this important part of venereal infection. He concludes this part of his work by an analysis of the late Mr. Hunter's opinion upon it. Having translated and published this work, I shall confine myself in this place to a statement of a few of the Authors, out of more than forty, on the symptoms of Lues Venerea, and the cure of it in new born infants.

BOERHAAVE, in his APHORISMS, remarks, as a general rule, that the Lues Venerea is transmitted through the means of the organs of generation as

Doctor of the Faculty of Medicine, Chief Physician of the Hospital du Vaugirard for Venereal Patients, and Member of the Society of the School of Physic. He was perfectly conversant in Greek, Latin, and English; he translated Black's Medical Observations from the English, and Stoll's Works from the Latin. He was author of a number of interesting articles in the Encyclopedia. His manuscript was prepared twelve years ago. His work upon *le Medicine Clinique et les Maladies Syphilitiques dans les Femmes Enceintes, dans les Enfants Nouveaux-nés, et dans les Nourrices*, has arrested my attention. I was struck with the justice of his remarks upon the subject of Lues Venerea, and therefore thought I could not do better than give them to the public. The other parts are not so much within my province. It is understood that Dr. Mahon died of a complaint in his breast about the year 1800, in the forty-eighth year of his age; and that Dr. Lamaue, Professor of Anatomy and Physic, Physician of the Military Hospital, Prévôt of Ecole-Pratique of Paris, and member of many literary societies, superintended his deceased friend's publication.

well as by sucking. There is nothing wonderful, says this celebrated author, that if the children whose father and mother are infected, that they should be born with the same disease. Indeed, since the venereal disease mixes with all the circulating humours, and is probably carried to all parts of the body before it arrives at certain parts, the foetus living in the womb of the mother is perpetually receiving vitiated humors, and there is perhaps already a morbid poison in the uterus before it is impregnated. On the other side it has been remarked, that a child which has been healthy while in the uterus of the mother, may be born infected with venereal disease, in consequence of its remaining a short time in its passage through the vagina, whence it gets impregnated with ichorous and purulent matter that it meets with there. It is in this manner that midwives and accoucheurs are sometimes infected, they having suffered the infection to remain upon their hands some time. In this case the infection of the child is not long before it shews itself, but in the other we cannot doubt but that the child may grow to an adult stage before it exhibits any marks of the disease*.

In the year 1736, the work of ASTRUC appeared, which makes a very remarkable epoch in the history of Lues Venerea. According to this author the disease is hereditary, and may be transmitted to the foetus equally through the father or the mother ;

* Van Swieten in Aphor. Boerhaavii.

by the father in consequence of the particles of the semen communicating to the embryo the venereal virus of which it is infected, and by the mother in consequence of her supplying, during the nine months gestation, the nourishment of the foetus—and she gives at the same time the disease with which she is infected. It is thus that it is proved by experience, that a mother who is infected brings into the world very weak and feeble children, of a bad constitution and half rotten, covered with ulcers and truly diseased; by the same means a father who is infected, begets children sometimes truly diseased and covered with ulcers, although the mother may be perfectly healthy; at least has no symptoms of the virus that infects the *embryo†. In another place, he says, that if a child while sucking has the disease, and that they do not apply any remedy in consequence of its age, yet as the nurse will no doubt be infected from the child, mercurial frictions may be applied to the nurse, and by this means the infant will be cured at the same time as the nurse by her giving it her milk. She will give, with her milk, particles of mercury which will be necessary for its cure; but if the child is weaned, they treat it without delay with mercurial preparations, or what is better, mercurial frictions. Its tender age should be no objection, for it is easy when used with prudence to prevent any bad effects‡.

* The observation of Astruc is true and important. J. F.

† Astruc, *édit Française*, par Louis, tom. 2 p. 5, 6.

‡ Astruc, tom. 4. page 192.

CHAPTER XVI.

THE DIFFERENT AUTHORS WHO HAVE WRITTEN SINCE THE YEAR 1750 UNTIL THE PRESENT TIME.

ROSEN, a Swedish physician, who published, about the middle of this century, salutary instructions upon the diseases of children, could not forget a subject which was so important as Lues Venerea in new-born infants, and it may be said that he was the first that wrote with precision and correctness on the principal symptoms which characterize it. It was some time before the wise precepts of this physician, upon these diseases of children, were known, in consequence of their being published in the Swedish language, and we should have yet been deprived of their knowledge, had it not been for the care of de Lefebre de Villebrune, who gave us a translation a little time before the death of that author. We will hear the opinion of Rosen, because what he says upon this subject is equally good.

If the father and mother are both infected, it is very natural that the child should be so also. As when the virus during pregnancy becomes more acrimonious or less virulent, in these cases the effect we find may be different. If it is extremely acrid or virulent, the child generally dies in the womb of

its mother. It is this which occasions also miscarriages. The child is sometimes born at its full period of utrogstation, but covered with ulcers which evidently prove the disease of which it is infected. Sometimes the progress of this disease is impeded by medicine that may have been given them, but has not radically cured them. In this case, the children have not the decided symptoms of the venereal disease. The virus with them appears unnatural, but it produces ills much more untoward, and they do not suspect the cause. These children grow but little, are weakly and sickly, and if they live to the adult age they only have a posterity which is very soon extinguished. It often happens that a child born of parents infected with the venereal disease, that there are no symptoms which prove its existence in them, while in others its existence is proved, the causes of which are, that the first are conceived and born before the venereal virus was in action, and the other is produced by the contrary effect. "Those children that are born of infected parents are thin, have little rest at night, are subject to some sort of eruptions from their birth, others at the end of six months. They have tumors in the groin, in the arm-pits, the ears, excrescences from the bone of the lower jaw or head, the bone of the arm, or from the shins. There is a continual discharge, as if from a cold in the head, a continual hoarseness without any apparent cause, a difficulty of swallowing, and you find in the throat

ragged ulcers of the appearance of bacon. You find a number of purulent pimples, scales upon the head and upon the forehead: there appear many red blotches, yellow about the neck and breast; at the lower part of the belly, chaps; chaps upon the hands and feet; excrescences, warts, and chancres upon the private parts. These chancres are sometimes small and sometimes large, sometimes smooth, other times hollow, generally of a deep red near its edges, and more or less hard: they are white in the centre, and become livid or blackish when they have existed some time: it is of a very different species to cancerous ulcers, beginning quite differently; it begins from a little unnoticed pimple, which soon becomes very painful: this is followed by little white pimples, which break and suppurate. Sometimes they appear like warts, that discharge white pus, which becomes yellow upon the linen."

In perceiving the precision, the justness, and the truth of Rosen's description, the physicians who were in the habit of seeing *Lues Venerea* in young children, were astonished that he did not place among his symptoms, the symptom of venereal ophthalmia, which will be proved hereafter to be the most remarkable and most frequent symptom among the phenomena, by its variety and its effects. Rosen adds, with much sagacity, that all these symptoms do not appear upon the same subject; the one may have more, the other less. It is much more easy to cure the disease in children, while

sucking, than when weaned. But the disease is much more obstinate, when hereditary, than when infected by the nurse. The sooner the disease exhibits itself, the more easy it is of cure. When an infant is born with this disorder, it may be cured by the milk of the mother, provided she uses mercurial frictions, but for the want of the woman, you may make use of a goat, which they must rub with a pomatum of mercury.

LEVRET* has said but little upon Lues Venerea in new-born children, but what he has said deserves to be noticed. In this manner, he explains how to act, in a book that he published in the year 1753: "No one doubts, at this time, that a child may be perfectly cured in the womb of an infected mother, if she is properly treated during her pregnancy. It is no less true, that if a child is born of an infected mother, that the child will bring the disease with it at its birth. It is also proved very evidently, that if a mother, infected with the venereal virus, suckles her child, and is treated as soon after her lying-in as can be done with propriety, both will be cured at the same time. The disease, if not manifested very soon after the birth of the infant, generally exhibits itself by ragged aphthæ with hard elevated edges, and which are communicated from its mouth to the nipples of the nurse, and there form Chancres. You necessarily administer immediately

* This popular Accoucheur must have seen a great many cases. J. F.

mercurial frictions to the nurse. In not doing it to the nurse, I have treated goats in the same way, which in suckling the infant has perfected its cure, as an ordinary nurse after undergoing the same treatment would have done. It is remarked, that in the different plans that are made use of for the cure of adults they accord exactly, and that the greater part of these unfortunate children die dropsical, either during the treatment, or a short time after*.

FABRE, a pupil of L. Petit, an excellent and celebrated Surgeon, and who rendered himself as celebrated as his master in the treatment of the Lues Venerea, published for the first time, in 1750, the opinion of Petit and his own upon Lues Venerea of new-born infants. "Children," says he, "are liable to be infected with Lues Venerea, either by generation or by contagion. It is the father or the mother who are diseased, that infect their children in the first case; in the second case, it is an infected nurse, who, suckling an healthy child, communicates to it the disorder. Children who are born with the disease are infected in different degrees, according to the circumstances in which they are begotten." "This remark," adds Fabre, "which influenced him in his practice, is due to L. Petit: according to this excellent observer, that when the father and mother, who were infected with this disorder, beget a child, it ought to have the disorder in an

* *Levret Arts des Accouchemens*, 1753, p. 266.

eminent degree, by its not only being nourished by an infected mother, but it is formed by semen also infected by the virus ; but when the mother is alone infected, the child ought to have the disease in a lesser degree than in the preceding case, in consequence of the semen of the father which contributed to its formation not being infected with the disease. We ought also to suppose, that if the mother is healthy and the father is infected, that the child will be less affected with the disease, as neither the semen nor the ovarium of the mother have contributed to the formation of the disease, as also in consequence of the subsequent existence in the womb it does not receive any new stimulus to the virus. In a word, supposing the father or the mother are not infected till after the formation of the child, it is certain that the child will be less affected with the disease than in the other case, at least it will be much easier of cure, because the semen that formed the child was uninfected with the disease, the disease not being so deeply rooted, and in consequence of which ought not to be so difficult of cure. If the disease appears in the child at his birth, you must hasten to stop its progress, and to cure it, if possible. If, in the case of a child which is too young for the administering to it the mercurial frictions, the making use of them on the nurse has been done with success, by the specific passing with the milk into the blood of the child, mercury may act effectually in eradicating the disease. But this indirect treatment requires

particular attention in relation to the milk of the nurse. She will lose her milk if she is suffered to bathe, or take too strong purgations, or too often repeated, or if salivation is induced. In consequence it will be necessary to use the mercurial frictions from time to time, and to prescribe her a regimen which is adapted to her situation. If it is the mother that suckles the child, she may use mercurial frictions, without any preliminary preparation, a few days after her lying-in. If the mother is not in a condition for the suckling of the child, or will not do it, there remains but one method for the safety of the life of the child, which is to nourish it with the milk of an animal. There is a plan proposed, which is, to suffer the child to be suckled by a goat, and to make upon its thigh a simple wound *, and to rub upon the wound every day a quantity of mercurial ointment. I prefer in this case to apply the ointment immediately to the child, either upon the tumours or upon the venereal ulcers it may have, or to rub twenty or thirty grains upon any other part of the child."

The evidence I have brought must accomplish more fully the nature of the subject; therefore, I now take my leave of it, with only making one more observation, that in the case I gave I was attending the father of the diseased infant at the instant of time the child was born with venereal infection, and

* There is no occasion for making any wound. J. F.

that nine months before, although from the history of the case, the father was then possessed of Lues Venerea, yet there were not then on him sufficient marks of it, so as to induce the father of the child to submit his case to the inspection of a Surgeon. The purity of the mother I know to have been unquestionable.

[A similar case, in every respect, was treated successfully after the same manner by me.]

The strange blunders and fantastic fictions which some medical writers sometimes put in print only excite a smile. Their reveries, and their exaggerated narratives, have a whimsical air of romance, in which truth is set boldly at defiance, but where deception leads to no serious consequences. Far different is the feeling awakened in the mind by erroneous accounts of the present state of Lues Venerea. On a subject so sacred to every sympathy of families, errors assume the character of calumny, and of calumny so much the more odious, as it seems in its tendency to bruise those already suffering. Let them recollect, that justice is not done, unless they be suspected, disputed, tried, and proved, as well as claimed and professed.

The interval of time, from the first contraction of a Chancre to the breaking out of primary symptoms of an infected constitution, will not pass without sexual connection, and that sexual connection, when pregnancy is a consequence, will not pass without that disease possessed by father or mother being,

with all other qualities of parents, conveyed and passed to a begotten fœtus. Every body, every fool, can change the mode of treatment of any disease; but the wisest among men cannot controul the natural and original law which governs a disease. Let them, if they can, alter the quality of the bite of a mad dog, and yet there have been similar writers upon that subject also, who have had the impudence to deny the reality of it.

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